SCIENCE

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*ARE FURTHER EXPERIMENTS NEEDED FOR DETERMINING THE ATOMIC WEIGHT OF OXYGEN?**

THE precision of experiments on the atomic weight of oxygen has been gradually so much increased that, in some cases, the mean error of a single determination is less than 1 part in 10,000. The agreement of different series of experiments is not so good, but if the work of different experimenters agreed well, the question, how accurately do we really know the atomic weight of oxygen, is not one which we can readily answer. Neither the concordance of the experiments of a given series, nor the agreement of the results of series of experiments by different observers, can excuse us from search for sources of error. All sciences which have to do with measurement afford sufficient instances of the fact that our conclusions are to be received with a certain suspense of judgment. And chemistry well illustrates that he is wise whose assertions regard the possibility of finding at some time evidence to the contrary.

The history of experiment on the atomic weight of oxygen affords an interesting example of the fact that neither the concordance of individual observations nor the agreement of different experimenters proves that a measurement is right. To-

* President's address, delivered before the New Haven Meeting of the American Chemical Society.

ward the middle of the century, Dumas made his classic experiments on the composition of water. The probable error of a single experiment was, in round numbers, 1 part in 400, so that the probable error of the average of the 19 famous experiments was 1 part in 2250. Now, this means that his final value was not likely to differ more than a certain small quantity from the result of the repetition of even a very large number of experiments made in the same way, with the same skill and care. But as to the difference between this result of the 19 experiments and the unknown true value, we are told absolutely nothing by the proposition that the probable error of Dumas' result was 1 part in 2250. It is a commonplace to say, that the calculation of the probable error of a series of experiments does not show how nearly the result approaches the truth, but how near it is to the result of a greater number of similar experiments. It decides, not how nearly we approach the desired goal, but whether it is useful to persevere by the present method of approach. Dumas made 19 observations, and got the value, 15.96, with a probable error of 0.007 ; that is, if he had made 100 or 1000 experiments, it is unlikely that the final result would not have been between 15.95 and 15.97, and very unlikely indeed that it would not have been between 15.94 and 15.98. But he would never have obtained a value near that which now commands confidence.

It is interesting to recall that there is hardly any instance on record where the judgment of an experimenter as to the degree of approximation to the truth attained in his work has been better justified than in the case of Dumas' classic experiments. As we all remember, towards the end of his work, there was discovered in his own laboratory a source of error, not easy to eliminate, which had affected all his determinations. The amount of the error was not a fixed quantity, and no numerical correction could be applied to the results of observation. Dumas accordingly gave to the public the uncorrected and unmodified results of experiment. But he also stated his opinion as to the degree in which his results approximated, not to the mean of a larger number of experiments of the same kind, but to the unknown and unattainable true value. He expressed the hope that his value would be found not more than one part in 200 from the result of those subsequent experiments which should be thought satisfactory ; and it is by just 1 part in 200 that his value differs from that which is now accepted.

So the concordance of Dumas' experiments did not prove that his result was right ; neither did the agreement of experiments by different observers. Erdmann and Marchand made eight experiments by a method like that of Dumas, with some modifications. Their result was 15.973, with a probable error of 0.011. This value differs from that of Dumas by less than the sum of the probable errors, so that that agreement is perfectly satisfactory. So, also, Regnault determined the ratio of the densities of oxygen and hydrogen, from which was computed the atomic weight of oxygen as 15.963, with a probable error of 0.004. The results of Dumas, of Erdmann and Marchand, and of Regnault, show a very good agreement. But all of them, and the mean of all of them, we now know to be in error by 1 part in 200.

I adduce this example, somewhat in detail, to enforce the proposition that we must not excuse ourselves from looking for error because observations agree. We have experiments which give the atomic weight of oxygen with a probable error of 1 part in 50,000, but do we know it within 1 part in 1000? Each individual experimenter whose work would now be regarded as free from known and tangible error, agrees

fairly well with the mean of all. For instance, Noyes' results show that degree of concordance which would justify us in expecting that, if he were to make 100 or 1000 experiments, his final mean would be as likely as not to be larger or smaller by 1 part in 9500, and his result differs from that which we accept by 1 part in 900. So Cooke and Richards assign a value which is just as likely as not to be within 1 part in 8000 of the result which they would have obtained by multiplying observations; and it differs from that which we accept by 1 part in 1500. But do we know that their means, and the means of all published results taken together, are not in error by 1 part in 900? The concordance of the results of a single experimenter, and the agreement of different experiments, does not justify us in asserting that we do.

In determining the atomic weight of oxygen, it has been somewhat difficult to determine directly all three of the quantities involved, and so to make what Stas called a *complete* synthesis. Berzelius, Dumas, and Erdmann and Marchand, weighed oxygen and weighed water, thus determining hydrogen by difference. More recently, Dittmar and Henderson and Leduc used the same method. Cooke and Richards, and Keiser weighed hydrogen and weighed water, while Rayleigh and Noyes weighed hydrogen and weighed oxygen. Any proceeding which weighs hydrogen directly has a great advantage in precision; different determinations in a given series agree better among themselves, and the series of different experimenters also agree better. But there is also a second, more important advantage. We have reason to believe that the constant errors involved in weighing hydrogen are small, for it is possible to obtain hydrogen with less than $\frac{1}{10000}$ or even $\frac{1}{20000}$ of its weight of impurity. There is no difficulty in weighing oxygen or water with accuracy, so if we weigh hydrogen and also weigh either oxygen or water, we may hope for a near approximation to the true value of the ratio sought.

We may hope, but we cannot *know*. We may believe that our hydrogen is pure, and that there was no error through leakage. But an unsafe stop-cock might make the apparent weight of the hydrogen in a series of experiments seem always smaller than the fact, and might yet leave the individual experiments so concordant with each other as to seem trustworthy.

If, however, we can weigh hydrogen and can weigh oxygen, and then combine them and weigh the water produced, we can at least give a better reason for our hope, if we find that the product is nearly equal to the sum of the components. The manipulation in this case is costly, and is so difficult, and involves so many minute details, that not many have patience and time sufficient for it, so that no great number of such complete syntheses has been made, and these few were made in conditions but little varied. When such complete syntheses shall have been made by different observers, with those variations of apparatus and method which may seem wise to them, we shall be able to judge of the magnitude of the errors to be feared. If such results are not concordant, we shall have much to learn as to sources of error; but we now see some reason to expect that they will not be discordant. However, even if they are not discordant, we are not to excuse ourselves from further study of sources of error.

Before repeating determinations so troublesome, and before studying unknown sources of errors not yet detected, the experimenter should receive all possible assistance from chemical theory and from criticism. For some, that criticism may be most profitable which is friendly and sympathetic; but, for the experimenter, the value of the

criticism depends on the knowledge and the acuteness of the critic. Dr. Hinrichs published, some five years ago, a criticism of all determinations and computations of atomic weights since Dumas. As is well known, he is a most strenuous and insistent supporter of Prout's hypothesis. Looking hastily through the volume, there was found, towards the end, evidence that its author was one of those who, some thirty years ago, discerned that which, in the hands of Mendeléeff became the periodic law. It seemed possible that one who had early seen some indications of this law might, perhaps, also have discerned, even if obscurely, some principle relating to atomic weights. I therefore once spent some time and pains in carefully reading the book, and considered at length those passages, which, if any, contained valid criticism of the views which are generally accepted.

Hinrichs believes that the mean of a series of determinations of an atomic weight cannot give the true value sought. This proposition he deduces from a mathematical discussion. He believes that as larger and larger quantities are taken in our analytical operations, the results differ regularly from ideal accuracy; sometimes the difference continually increases as the quantity taken increases; sometimes the difference increases to a maximum and then decreases again. The proper computation of an atomic weight then, according to Hinrichs, consists not in taking the mean of different observations, made with different weight of materials, but in determining the limit towards which the series converges as the weight taken decreases. A good illustration is given: we cannot determine the weight of a new coin by weighing any number of old coins; every coin is worn and therefore light, and the mean weight of any number whatever is therefore necessarily below the mean weight of new coins. But if we weigh old coins and note the date of each, we may take the mean weights for each year separately. If we examine coins enough, these means when plotted as the ordinates with the years as the abscissas, will give us a "fairly regular curve, lowest for the oldest coins, gradually rising towards a *limit* which they would not quite reach. This higher limit would evidently be the mean weight of the new coin."

This is an intelligible proposition. It seemed to me worth while to examine it, for to this proposition one of the most enthusiastic and most active supporters of Prout's hypothesis, a man not lacking in shrewdness or ability or learning, has entrusted the defense of his favorite belief.

He asserts that an atomic weight as determined by experiment is variable, that it depends on the amount of substance taken for the analysis or other operation, and that it varies in a continuous and regular manner. His proposition is, that an atomic weight as determined by experiment is a function of the weight of substance taken. Is there any evidence in favor of it?

I answer, first: Theory does not afford any evidence for it. Hinrichs deduces this proposition from theory by a discussion which is mathematical in form. Whether the proof is sound need not be considered, for his theory does not attempt to show the order of magnitude of the regular and continuous variations which are affirmed to depend on the weight of substance taken, and to show whether they can be separated from the irregular and discontinuous errors due to accident. We are sure that accidental errors exist; we may concede for argument, that regular and continuous variations also exist; but this is far from implying that the actual errors in a given set of experiments will be largely or even perceptibly of the latter kind. Theory shows that there is a diurnal tide in the atmosphere; but theory does not show that the differences noted in

a series of ten observations of the barometer at different hours of the day will follow the law of the diurnal tide.

But, secondly: Facts do not agree with the proposition. The accidental errors of the most precise experiments yet made are so much greater than any *systematic* variations, that nothing but accidental variations can be detected. To prove this, let us consider Stas' synthesis of silver nitrate from pure silver. This is one of the most important determinations ever made; Hinrichs has discussed it twice in his volume; he asserts that the ratio of silver nitrate to silver found in each analysis depends on the weight of silver taken, and twice draws curves to show this. I assert, on the contrary, that the errors, which average only 1 part in 40,000, are purely accidental, and that they follow no law. This can be proved by proving that the ratios obtained depend on any other quantities selected by accident just as much as they depend on the weights of silver taken. Hinrichs plots the results of the ten determinations, using for abscissas the weights of silver taken, and so obtains tolerable curves. I plotted the same ten observations, using for abscissas not the weights of silver taken but ten numbers selected by *sortes Virgilianæ*, and get curves quite as tolerable as before; and this I did with ten different sets of abscissas, all selected by pure accident. Now, quantities which depend on any one of eleven sets of abscissas, ten of which are selected by accident, are themselves accidental in their variations, and the variations follow no law. So far as the facts are examined, they give no evidence in favor of Hinrichs' proposition; we have seen that theory is equally chary of her support, and we may safely dismiss the suspicion that any source of systematic error can be detected in deducing atomic weights from the means of good experiments.

Within the limits of convenience, it is well to vary the amount of substance taken in analytical determinations. This has been a frequent practice in the finest investigations. But that this practice derives any support from the so-called 'limit method' cannot be conceded.

This criticism, this attempt at a theory, then, removes no obstacles and reveals no threatening pitfalls. If we desire a firmer foundation for our system of atomic weights we must simply enlarge the experimental basis of our knowledge.

If it is only by further experiment that we can make surer of the atomic weight of oxygen, we are to consider what kind of experiments is most desirable. It is chiefly for the sake of eliciting discussion on this point that the subject of this address has been chosen.

Our present value for the ratio between oxygen and hydrogen rests on one single chemical combination, and upon two processes for determination. The first is, the synthesis of water from its components. The second is, the determination of the densities of the gases and of their volumetric ratio. Let us consider these in order.

What synthetic experiments should be repeated? We are met by the fact that a complete synthesis, in which hydrogen and oxygen and water are all three weighed, can be made with errors only about one-fourth as large as the errors in any of the syntheses where only two substances are weighed out of the three concerned. Other things being equal, then, this process is by far the most promising. But, so far it has been carried out adequately by only one experimenter. Conditions were varied somewhat, it is true, but by no means so much as they would be varied if the same person repeated the experiments after an interval of years; by no means so much as if others were to undertake such complete syntheses. Lately, Keiser has devised a process which varies in many particulars from that already

executed; it is very desirable that he should make a series of experiments, after adequate study of sources of error and of means of avoiding them. It is also desirable that, if possible, the original process of complete synthesis should be repeated with the little modifications which time is sure to introduce. These two would be enough, as far as synthesis is concerned; unless, indeed, through the invention of another process by a third experimenter, we could have still more. Other syntheses of water than by a *complete* synthesis seem less likely to be of much service, except as a school of experimentation.

What further work is desirable on the ratio of densities and of combining volumes of hydrogen and oxygen? Three constants are involved: the density of oxygen, the density of hydrogen, and the volumetric ratio.

The density of oxygen is known with a probable error of about 1 part in 50,000. It is very probable that no number whatever of further determinations would change this value by 1 part in 10,000. No further work upon this density seems at present desirable, except that whoever determines the density of hydrogen cannot well fail to determine that of oxygen also.

The density of hydrogen demands further experiment. It is possible to make, by some one of three or four slightly different processes, a series of experiments whose average variation shall be less than 1 part in 3000 or 5000, or even 10,000; but different series do not agree sufficiently with each other. We are far from knowing the density of hydrogen so well that more observations might not change our value by 1 part in 2000 or 3000. It is very desirable that further observations should be undertaken by at least two different methods. In one method, hydrogen should be weighed while absorbed in palladium, should be then transferred to a measuring apparatus without the use of stop-cocks, and should be there measured. This process should be repeated with measuring apparatus of varied volumes. In another method, hydrogen should be weighed after Regnault's method, in a counterpoised globe, but with such precautions that leakage through a stop-cock, and contamination with vapor of mercury, should be excluded. The globe should be exhausted till the remaining air is a small fraction of a millionth, should be sealed off from the pump, and should be connected with a condenser at the temperature of liquid air, so as to remove mercurial vapor. After this hydrogen is to be admitted without the use of stop-cocks. The manipulation is not difficult, and the method would confirm the results of the previous method.

The ratio of the combining volumes of hydrogen and oxygen is not known with the degree of confidence which is desirable. The history of the matter is not an uninteresting one. Further continuance of the two series of experiments on which the present value depends would be most unlikely to change it by 1 part in 10,000, for its probable error is 1 part in 40,000. But one of the experimenters has obtained results differing from that finally adopted by as much as 1 part in 220. The other experimenter has entirely discarded the result of one series and replaced it, not by a better series of the same kind but by one of a quite different nature, not carried to its proper completion, and accordingly reduced by the use of the constants of van der Waals' equation. It is desirable that experiments be made to furnish means for a new reduction by measuring the change of volume when 2 volumes of hydrogen and 1 volume of oxygen are mixed, being at the same pressure before and after mixing. This experiment has lately been made by Berthelot, whether with sufficient precision for the purpose is not known at this moment. It is also desirable that the ratio of the combining volumes of

oxygen and hydrogen should be measured with the gases contained in vessels of the dimensions of those used for obtaining their densities.

If these syntheses and these studies of ratios of densities and combining volumes should agree as well as it is safe to expect, we should know the atomic weight of oxygen as confidently as we can know it while the value rests on a single chemical process, the combination of the two gases to form water.

But this is not so much as is desirable. We know the atomic ratio between silver and oxygen with considerable confidence, because this rests not on a single chemical process but on eight different chemical processes, which give eight independent results, and because these eight results agree. Is there, then, any chemical process by which the atomic ratio of oxygen and hydrogen can be determined, other than the analysis or synthesis of water? Is there any element whose atomic ratio to oxygen is well known, whose ratio to hydrogen is capable of accurate direct determination?

It is probable that, given an adequate equipment, the direct ratio of hydrogen to chlorine, of hydrogen to sodium, of hydrogen to magnesium, or of hydrogen to aluminum, could be determined with sufficient precision for the purpose, provided that the ratio of chlorine to oxygen, of sodium to oxygen, of magnesium to oxygen, and of aluminum to oxygen are well enough known. This may not now be the case with aluminum or magnesium, but is the case with chlorine and with sodium, whose atomic ratios to oxygen may be fairly assumed to be known within 1 part in 2500. If, now, we can determine the ratio of chlorine to hydrogen, or of sodium to hydrogen, to 1 part in 5000, we could compute, by a new method, the ratio between hydrogen and oxygen. If this should agree with the present value, within some such quantity as 1 part in 2000, we should be as confident of the truth of our value of the atomic weight of oxygen as we can well hope to be.

The difficulties in making a complete synthesis of hydrochloric acid are not small, nor are they all well understood. Some unexpected circumstance may be prohibitive. But there is good reason to hope that 3 or 4 or 5 grams of hydrogen could be weighed, that a nearly equivalent quantity of chlorine could also be weighed, that the two could be combined, and that the product could be weighed. One serious difficulty would be found in attempting to prepare pure chlorine, but the difficulty does not seem insuperable. The manipulation of the corrosive element requires invention, but seems not difficult. For the collection of the hydrochloric acid in a weighable form, there seem to be alternative methods, not very troublesome of execution, unless unforeseen difficulties are encountered. If the ratio between hydrogen and chlorine could be determined to 1 part in 5000 or to 1 part in 10,000, it would be a very interesting addition to our list of known constants, most helpful in establishing confidence in the ratio between oxygen and hydrogen.

So, also, if sodium can be prepared of sufficient purity, or of sufficiently constant impurity, it seems possible to weigh 100 or 200 grams, to act on with water in such a way as to produce a slow evolution of hydrogen, and to determine the weight of this hydrogen by loss. Whether sodium can be obtained sufficiently free from absorbed hydrogen and whether it can be prepared for weighing without attacking the vessels which contain it, are questions which need further experiment. It is probable that a vessel of platinum-iridium alloy could be made which would make success almost certain, but at considerable cost. In this case also, if the ratio between sodium and hydrogen can be

determined to 1 part in 5000, or 1 part in 10,000, the result would inspire confidence, or, if it must be, distrust, in our present value for the ratio between oxygen and hydrogen.

These suggestions, necessarily tentative in their nature, are submitted to the American Chemical Society, in the hope of obtaining from those who do me the honor to listen to them or to read them, expressions as to the desirability of making experiment in the lines described, and discussions of the new methods indicated as possible.

Edward W. Morley.

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CRUISE OF THE ALBATROSS.

III.

Mr. Agassiz's third letter written from the *Albatross* to Hon. George M. Bowers, U. S. Commissioner of Fish and Fisheries, is dated Suva Harbor, Fiji Islands, December 11, 1899, and is as follows:

We left Papeete, November 15th, after coaling and refitting on our return from the Paumotus. During our trip to Suva we made a few soundings from Tahiti to Tonga, striking the northern extension of the deep basin lying to the eastward of Niue; the depths ranged from 2472 to 2882, the bottom being red clay. This would indicate a greater extension westward of the zone over which the manganese-nodule bottom extends.

After leaving Niue we steamed for the deep hole of the Tonga-Kermadec Deep, about 75 miles to the eastward of Tonga-Tabu, and in 4173 fathoms made a haul with the Blake beam-trawl, by far the deepest trawl haul yet made. The gear was carefully inspected and strengthened as far as practicable by Captain Moser, and it was with considerable anxiety that we laid out 5000 fathoms of wire rope for our haul. Fortunately, everything went off successfully and we landed the trawl safely back on deck. To my great surprise we found in the bag a number of large fragments of a silicious sponge belonging probably to the genus *Crateromorpha* which had been obtained by the *Challenger* in the Western Pacific, but in depths less than 500 fathoms. We also brought up quite a large sample of the bottom; it consisted of light-brown volcanic mud mixed with radiolarians.

We decided to trawl at 4173 fathoms rather than wait for a possibly deeper sounding, as the conditions for work were admirable and we did not care to run any risk from a change of weather. After our haul we made a still deeper sounding in the proximity of the 4762-fathom sounding marked on the chart, and found 4540 fathoms with the bottom of the same character as at the place where we trawled. We also took a couple of soundings in the line from Vavau to the southern extremity of the Lau Group in Fiji, but found, as we expected from the soundings given further south, comparatively shoal water, viz., 1381 fathoms. In the channel north of Yangasá, where we crossed the Lau Plateau between Yangasá and Mothe, we found 453 fathoms, with bottom composed of coral sand, pteropod ooze and a few globigerinæ. Between Namuka and Yangasá we obtained 324 fathoms, between Namuka and Marambo 600 fathoms, and between it and Kambara 450 fathoms, and finally about 15 miles west of Kambara we sounded in 990 fathoms. These soundings would indicate a continuous plateau of moderate depths from Wailangolala south upon which the islands of the Lau Group rise.

On our way back to Papeete from the Paumotus we examined the eastern coast of Tahiti, and from Papeete examined the western coast as far as Port Phaeton at Tararao Isthmus. We examined in a general way the Leeward Society Islands: Murea, Huaheine, Raiatea, Tahaa, Bora-Bora, Motu Iti and Maupiti. There are

excellent charts of the Society Islands, so that it was comparatively simple to examine the typical points of the group and to gain an idea of their structure as far as it relates to coral reefs. The Society Islands are all volcanic islands edged with shore platforms, some of great width, upon which the barrier or fringing reefs of the islands have grown. The structure of the reefs of the Society Islands is very similar to that of the Fiji reefs round volcanic islands. A comparison, for instance, of the charts of Kandavu, Viti Levu, Mbengha, Nairai, and and of other volcanic islands in the Fijis, with those of the Society group, will at once show their identity. Huge platforms of submarine denudation and erosion characterize both, with fringing and barrier reefs determined by local conditions. Perhaps it is easier to follow the changes which have taken place in the Society Islands; and such islands as Tahaa and Bora-Bora, where we anchored, as well as Maupiti, are admirable examples and epitomes of the structure and mode of formation of the coral reefs of that group.

In Motu Iti and Tetuora the volcanic peaks have disappeared, leaving nothing but a shallow platform upon the outer edges of which sandy coral islets have been thrown up. There is, however, one point in which the barrier reefs of the Society Islands differ from those of Fiji. The barrier reefs in Fiji are generally indicated merely by reef flats, upon which the sea breaks, and an occasional rocky islet or negro-head; only rarely do we find sand keys upon the fringing reefs of the islands of Fiji. In the Society Islands, on the contrary, we find the line of the barrier reef usually well indicated by long lines of narrow islets thrown upon the reef platforms, exactly as they are in the Paumotus. These islands and islets are usually well wooded, and thus give a very peculiar aspect to the barrier reef. In the case of Bora-Bora, Maupiti and Aitutaki, for instance, we have a central volcanic peak of considerable height surrounded by a wide lagoon, the sea edge of which is formed by a fringe of wooded islets and islands forming a more than half-closed ring around the central island which, in Bora-Bora and Maupiti, rise in slopes and nearly vertical walls, the former to a height of nearly 2400 feet, the other about 1100 feet.

The only island of the Cook group which we examined was Aitutaki, as Atiu is composed of elevated limestone, and Rarotonga is volcanic. I hoped we might find that atoll to be in part volcanic and in part composed of elevated coralliferous limestone; we found it to be volcanic, an island with the structure of Bora-Bora on a smaller scale.

We anchored at Niue, an island composed of elevated coralliferous limestone showing three well-marked terraces, the lowest of not more than 5 to 10 feet and in many places disappearing completely, the limestone cliffs rising vertically from the sea well into the second or even the third terraces. The vertical faces of the cliffs are dotted with caverns and deeply indented by small cañons extended at right angles to the face of the shore or forming blunt headlands separating short reaches of coral sand beaches. The second terrace varies in height from 50 to 60 feet, the third from 90 to 100 feet. The second terrace is deeply undercut; and in the higher vertical cliffs extending into the third terrace from the sea, the former positions of the terraces are usually indicated by lines of caverns. There are corals on the sea slopes of the first terrace extending to 10 or 12 fathoms growing much as they are found at Makatea.

From Niue we went to the Tongas, which we found a most interesting group. The elevated tertiary coralliferous limestones take here their greatest development, and are on a scale far beyond that of their

development in the Lau Group of the Fijis or the Paumotus. The first island of the Tongas we visited, Eua, is perhaps the most interesting of the islands composed of tertiary elevated coralliferous limestone I have visited. From Dana's account of it, evidently given at second hand, I expected to find an island somewhat like Viti Levu on a very much smaller scale. But as we steamed up to it from the east there could be no mistaking the magnificent face of nearly vertical limestone cliffs forming the whole eastern face of the island, and at points rising to over a thousand feet in height. At all projecting points lines of terraces were plainly marked; at the northern point three could be followed, and at the southern extremity five, with perhaps traces of a sixth.

Upon rounding the southern extremity of the island we could see that the island was composed of two ridges, running north, separated by a deep valley, the western ridge being much lower than the eastern, not rising to a greater height than a little over 500 feet. The western ridge is also composed of limestone, and at the headlands we could trace three terraces. There is a narrow shore-platform along the western face, at many points of which there are blow-holes where the sea throws up spray to a considerable height, but these blow-holes are best seen off Cook Point, the southern extremity of Tonga-Tabu.

As we steamed along the western face of Eua Island, we could see the ridges of the eastern side rising above the crests of the lower ridge, its slopes indicating a valley of considerable size. We anchored at English Roads, opposite the outlet of the drainage of the interior basin where a small river has cut its way through a depression in the shore terrace. On landing we followed the crest of the western ridge for a few miles and could see the whole valley forming the basin of the island lying between the two ridges, at our feet; the slopes leading to the bottom are quite gentle, and the valley dips very gradually northward back of the outlet, on the western shore. Nothing could show more clearly that such an island was not an elevated atoll, but a plateau which has been eroded and denuded for a long period of time by atmospheric and other agencies, and in which a deep basin-shaped valley with gentle slopes has been gouged out—a plateau similar to that of Tonga-Tabu Island and of Vavau, but of greater height and less extent.

To the westward of the Tonga Islands is a line of volcanic islands extending nearly 200 miles from Honga Hapai to Fanualai, some of which have been active very recently. Falcon Island disappeared in 1898 and Lette is still active. This line of volcanoes runs at a distance of from 15 to 20 miles parallel with the trend of the four irregularly shaped plateaus upon which rise the Tonga Islands. They are the summits of a great ridge over 200 miles in length, sloping very gradually to the westward and being somewhat more steep to the eastward, into deeper water, towards the smaller platforms from which rise the volcanic peaks of the group. The plateaus of Tonga-Tabu, Namuka, Hapai, and Vavau, being separated by deep valleys connecting the eastern and western flanks of the ridge. These four plateaus rise abruptly from the 100 fathom line. The extremity of the southern one is occupied by Tonga-Tabu Island. The land behind the cliffs of its southern coast rises to a height of over 250 feet and slopes northward very gradually to form the low land which occupies the north coast of the island, and is, except, at Mount Zion and Cook Hill, not more than from 10 to 20 feet above the level of the sea. At Cook Point (and along the southern coast) three terraces are indicated. The northern coast is deeply indented by shallow bays, full of islands, reef flats, and reef patches, on

which corals grow in great profusion. In a distance of nearly 10 miles northward of Nuku-Alofa the plateau is nowhere more than 15 fathoms deep; and a long tongue runs northward, gradually deepening into 20 to 50 fathoms to the 100-fathom line.

The Tonga-Tabu plateau is separated from the Namuka Group Plateau by a funnel-shaped channel with a depth passing rapidly into 300 fathoms from the 100-fathom line. The Namuka Plateau is rectangular. The principal island is Namuka, where we anchored. We found the island to be composed of tertiary elevated coralliferous limestone with a shallow sink, filled with brackish water, occupying the southeastern part of the island. The sink is separated by a high sand beach, about 200 yards wide, from the sea.

Namuka Iki, the island next to Namuka, we found to consist, at its southern extremity, of stratified volcanic material resembling somewhat the so-called soapstone of Fiji. I was told that other islands in this group, near Tonumeia, in the center of the Namuka Plateau, were volcanic. Mango, as we could see it from our anchorage, appeared to be volcanic. So that this part of the Tongas is, like the Lau Group in Fiji, made up of islands in part volcanic and in part composed of elevated coralliferous limestone. The eastern edge of the Namuka Plateau (which we did not visit) is edged with small low islands. We merely steamed past the western islands of the Hapai Group, but close enough to see that Tongua, Kotu, and Fotuhaa, which are respectively 120, 120 and 200 feet high, are composed of elevated limestone. The eastern flank of the Hapai Plateau is edged with long low islands with extensive coral reefs along the reef flats of these islands.

The Hapai Plateau is triangular, with isolated islands rising on the northwestern side from the deep water separating it from the Vavau Plateau. It is separated from the Namuka Plateau by a narrow channel with over 300 fathoms of water.

The northernmost plateau of the broad ridge of the Tonga Islands is the Vavau Plateau. This is elliptical, with a long tongue extending on the eastern face of the ridge toward the northern point of the Hapai Plateau, ending in isolated banks (the Disney and Falcon banks), lying to the northward of the broad channel, with over 400 fathoms separating it from the Hapai Group. The Vavau Group is by far the most picturesque of the Tonga Islands. It consists of the principal island of Vavau, extending across the northern part of the Vavau Plateau. Several parts of the island of Vavau are finely terraced; four terraces are indicated there, and other flat-topped smaller islands show traces of two or three terraces. The northern edge rises to a height of more than 500 feet, and slopes in a general way southward and inland. The southern shore is deeply indented by bays and sounds and flanked by innumerable islands and islets, some of considerable height (150 to 250 feet), which gradually become smaller and smaller as they rise toward the southward and eastward, these islands having been formed from the denudation and erosion of the greater Vavau. They form tongues of land and sea and sounds of all shapes and sizes, showing the traces of the former land connections of the islands and islets and their disintegration on the eastward and southward by the action of the sea. The islands and islets to the southward of the main islands rise from more or less extensive reef flats which stud the whole plateau, and on which corals grow in great profusion (mainly Millepora, Porites, Pavonia, Pocillopora, Fungia, and Astrea), to a depth of 5 to 6 fathoms in the sound. In the Namuka Group they extended in the more open waters to 14 and 16 fathoms.

It is evident that in the Tonga Group, which is a very extensive area of elevation, the recent corals have played no part in the formation of the masses of land and of the plateaus of the Tonga Ridge, and that here again, as in the Society Islands and Cook Islands, both also in areas of elevation, they are a mere thin living shell or crust growing at their characteristic depths upon platforms which in the one case are volcanic, in the other calcareous, the formation of which has been independent of their growth.

We expect to leave for the Ellice, Gilbert, and Marshall islands as soon as we can coal and refit.

A. Agassiz.

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*THE OCCURRENCE OF APTOSOCHROMATISM IN PASSERINA CYANEA.**

The following remarks upon the Aptosochromatism of *Passerina cyanea*, although of insufficient importance to establish the phenomenon of color change without moult as a constant occurrence in the species, are conclusive enough, I am convinced, to prove the possibility of such a change, and are merely offered as such for what they may be worth.

Individual error and dogmatism have greatly retarded honest effort in this most important branch of ornithological science. It is a singular fact that certain individuals have conceived the idea that a feather once having passed its premature condition is utterly disconnected with the vital system of the bird, and such individuals cling to this belief with a tenacity wonderful to behold. They do not tell us, by the way, how it is that certain species of birds lacking external sebaceous glands manage to present as bright plumage as their allies so provided. Doubtless they may attribute the presence of oily matters upon the surface of the feathers of those species in which these glands are wanting to osmotic action; but admitting this, why not admit Aptosochromatism?

In his article on alleged changes of color in feathers (Bull. Am. Museum Nat. Hist. 1896), Dr. Allen compares a feather to a green leaf, which when once formed, cannot extend its growth to repair any injuries which may arise from insects, etc. This simile might well be carried yet farther and to better advantage. When the later summer or early fall approaches, certain leaves undergo a complete change in color, resulting in the beautiful colors of our September and October woods. The history of the underlying phenomena of autumnal coloration in leaves is very obscure, yet no one doubts the occurrence of the change for an instant. So it is with Aptosochromatism—the individual feathers undergo in many cases complete color changes, and although the underlying processes of these changes may be obscure, the fact of their presence is to my mind undeniable.

At the present time Aptosochromatism has not progressed far enough to encourage one to take up in detail the systematic occurrence of the color change in our species of native birds. It seems evident that for the present, attention should rather be devoted to endeavoring to demonstrate its fundamental principles, without which no science is firm, plainly evident as may be its happening.

Passerina cyanea, apart from its seasonal fall moult by which the plumage acquired in the spring is changed for the duller garb of the fall, doubtless exhibts two forms of Dichromatism, a term whose proper place, I hope, is now recognized as the fundamental term for the complex phenomena of double coloration. As I shall direct my attention toward proving that Aptosochromatism is concurrent in the species, and Ptosochromatism in the present paper will play an inconspicuous part. Both are compre-

* Read before the Nuttall Ornithological Club of Cambridge, Mass., June 5, 1899, with exhibition of the bird worked upon.

hensive terms, by the way, coined by Dr. Coues. The latter term may be defined for convenience, as the occurrence of Dichromatism depending entirely upon the loss of old feathers which are replaced by others of a different color. Both processes subserve the same general purpose and result in a seasonal change of color, in the present example, from that of the fall to the nuptial of the spring. The Ptosochromatic change of the spring will not further be considered here—let it suffice to say that beyond doubt some indigo birds change color in the spring by completely, or nearly so, shedding their feathers. The change not due to such feather loss is what interests us at the present, and will necessarily be presented far from exhaustively. My remarks are based chiefly upon observations conducted during the fall, winter, and early spring of 1898–99, upon a captive male bird. In view of the color of the plumage of my bird, at the time I took possession of it, it must have been in the adult condition, and as such birds are commonly captured while in the adult state, the difficulty in adapting my bird to his captive condition and the heavy feather loss undoubtedly resulting from this may consequently, be explained. I secured it October 13th, while it was nearing the completion of its fall moult, which by October 28th was finished. From that date every cast-off feather was carefully collected and labelled. In order to be sure of obtaining all, a fender was placed about the cage and the room carefully swept at frequent intervals. In addition to this I made many examinations of the bird and secured such specimen feathers in a fresh state as I wished for microscopic examination.

Briefly, the bird was fed mostly upon millet and canary seed, appeared healthy, bathed regularly, and during bright days sang frequently. On March 26th, however, he died after an illness of three days, before which he was as lively as ever. Examination showed constipation to be the probable cause of his untimely death. The color change had progressed excellently however, and but little additional information could have been gained had he lived, excepting the period of time occupied by the entire change. Data is present in sufficient quantity, nevertheless, to enable me to state the time occupied by the change in certain portions of the plumage.

The appearance of my bird as regards color, October 28th, was as follows: Feathers of the head and breast slightly tinged with cobalt, the chief color of the feathers being dull rusty. The breast thus was somewhat mottled in appearance. Back rusty, the concealed centers bluish. Primaries and rectrices blackish with blue edges. Secondaries and coverts broadly edged with rust color. Lower abdomen buffy, ventral area whitish.

From October 28th till January 28th I observed no marked color change, but from February until the bird's death, it was very noticeable. A curious and undoubtedly unnormal process intervened however, and in order to understand this comprehensively I will tabulate the feather loss beginning with the first feather shed after October 28th, when it will be recollected that the bird had completed its moult.

Nov. 1,	1	contour	
" 7,	1	"	
" 12,	1	"	
" 21,	2	contours	
" 22,	1	contour	
" 25,	2	contours	
" 29,	1	contour	
Nov. 30–Dec. 5,	4	contours	
Dec. 5–11,	4	"	
" 11–24,	20	"	
" 24–31,	12	"	
Dec. 31–Jan. 7,	11	"	
Jan. 7–23,	3–5	"	daily
" 24,	12	"	
" 25,	16	"	
" 26,	13	"	
" 27 and 28,	61	"	
Jan. 28–Feb. 11,	34	"	

From February 11th to February 28th an average of 50 contours was lost daily. By March 5th the loss had abruptly ceased, and until his death on March 29th the bird lost but one or two feathers daily. The loss of down feathers was very small, not exceeding twenty specimens—a fact of possible importance to be dwelt upon later on. The total summary of the contour feathers lost, carefully estimated at 1350 feathers, appeared to comprise about three-fifths of my bird's entire plumage. They were shed from all parts of the plumage, and in view of the heavy loss I was quite prepared to ultimately conclude that any color change resulting in my bird would ensue from extensive feather loss. I was thus quite unprepared for what eventually followed. Microscopically, the cast-off feathers were broken, abraded and apparently in the worst condition. The barbules were broken or wanting, the barbs in many places worn down, and the rhachis of the larger feathers was split. This is also of importance as will directly be seen. To many observers my bird by March 5th would have been pronounced to be completely moulting. Immature feathers were in prominence especially upon the head and were scattered all about the remaining feathers of the plumage, which, as has been before remarked, amounted to about two-fifths of the entire plumage. The color of a discarded feather, compared with a freshly plucked one from the body, showed in most cases a decided contrast. Not only were the blue portions dull but their superstructures were gone in many cases, the feathers then being dull brownish. A probable, and, as I am convinced, the truthful source of my bird's extensive loss is found indirectly in the temperament of the bird. From the very first it was fretful and timid, fluttering wildly when uncovered in the morning or when the cage was cleaned out. Even an approach in his direction while hanging in the room caused a wild fluttering. Upon such occasions many feathers would be shed, and those remaining were more or less injured. Thus when such a vital process as Aptosochromatism begins to work, these decrepit feathers necessarily would have to be renewed in order to take part in the general plan. The head upon which many pin feathers appeared, naturally received a considerable share of the injury as regards its feathers, and the tail was in a very bad state.

It will be noticed in the table how gradually the loss began, due doubtless to the gradual approach of activity towards color change in the feathers. It must be admitted that this explanation is purely hypothetical, but such a hypothesis, although not of fundamental importance, ofttimes prepares the way for a clearer understanding of the problem under consideration. The small loss of down feathers points in two ways to the truth of this assumption. Firstly, being more or less under the contour feathers they received less of the wear caused to the others, and secondly, having no color change in themselves, their part in the color-changing process was inconspicuous. It is not probable that the bird could have swallowed many down feathers without it being observed in the excrement.

As I frequently examined the bird closely I noticed at once that the developing feathers which were supplying the places of the cast-off ones, far from appearing to change the color of the bird to blue, were actually coming true to the colors in which they were shed, *i. e.*, in the colors of the fall plumage. To be positive I collected and examined extensively and in every case verified this most interesting principle. It will be recollected that in my observations upon the Aptosochromatism of *Chrysotis Levaillanti* (see *Osprey*, III., No. 8, April, '99), similar results were noticed. In later dates a few parti-changed feathers were

found in the embryonic condition, but these may be readily accounted for in two ways, for in no cases were fully changed feathers so detected. (1) Where a partially changed feather had been pulled out or shed and was being renewed, and (2) where a feather had begun to change before it was matured (this being noticed in my parrot investigation). Many of the contradictory and confusing remarks of dealers in birds may perhaps be explained by these most important observations, and it may readily be true that more than a few instances of so-called color changes depending upon spring moults in cage birds, may be due to extensive feather losses precedent to an Aptosochromatic change.

It seems unnecessary to dwell upon the fact that no vital process can readily take place in a greatly injured organic structure, and the renewal of my bird's feathers concurrent with the approach of the macroscopic activity of the color change is, I think, an incident of no little value.

The first appearances of a color change were noticed in some of the old feathers of the crown during the first week of February. Here a brightening of the blue area of the feather was noticed but no perceptible change of color at the tips where the russet was. From this date till the death of the bird a slow but constant change occurred, chiefly noticeable on the rump, throat, and breast. The first indication of the approach of the change externally was the brightening of the blue portion of the feather, beginning evenly on each vane from the bottom. When the band of tawny was reached, it appeared slowly to be absorbed until but faint tips of this color could be seen upon the ends of the larger barbs. In no cases were the barbs or barbules broken off sufficiently to account for the change. A loss of one-third of the length of the feathers in many cases would have been the result, and close observation did not sustain this in the least. I was enabled to notice the change in certain breast feathers, which was much more rapid than that of the parrot before referred to. Yet in the latter case the change which gradually causes the yellow plumage is a slow one of life duration, and but few feathers are involved at a time. That of *Passerina cyanea* is one of comparatively short duration and involves the greater part of the entire plumage.

Upon his death the bird presented the following appearance as regards color. An irregular area of brilliant cobalt blue extended from the throat to the belly down the center of the breast, the feathers upon each side graduating gradually to the sides, where but little change had occurred. Head partially changed in parts, inter-scapulars not perceptibly changed at all (a place where many pin feathers formed). Back altered slightly, rump and scapulars $\frac{3}{4}$ changed to bright greenish blue, the long russet tips almost entirely changed in some feathers, wings and tail unaltered with the exception of the secondaries and coverts, which had slightly changed on the edges. Throat slightly blackish, lower ventral region as with the rump. Assuming, as we safely may, that the first of February marked the beginning of the macroscopic change, we may attribute the total length of time occupied in the change of some of the breast feathers, from the fall to the spring colors, to a period of about fifty days, which for some of the feathers is an over-estimate.

It is not a little curious that the feathers should act so independently and especially so when it is considered that each feather comes true in color.

While skins of birds may serve highly important purposes, it appears essential that for good results in investigations upon color change, one should operate rather upon live birds in confinement. It is quite ab-

surd to suppose that a single generation in confinement would so alter the natural laws of the organism as to obscure Aptosochromatism or Ptosochromatism, if one process or the other be a natural tendency. It is objectively certain that the phenomenon of Aptosochromatism occurs widely, but whether of individual or specific occurrence is not yet clearly shown. While my bird threw out no hint whatever as to the constant occurrence of the color change, it did prove that the 'impossibility' is possible.* It is certain that the heavy feather loss of my bird but indirectly helped the change; 1st, we have seen that many feathers changed which were not renewed by moult; 2d, we saw that those feathers which were renewed by direct gain and loss were colored similarly to those which preceded them, but that later on they changed Aptosochromatically, and 3d, no purely blue, *i. e.*, changed feathers, were found in an embryonic condition at any time, although frequent careful examinations of the bird were made.

Although it is of no positive certainty whether, in the new feathers, the vascular connection with the body was severed, it was found that their complete form was attained in most cases a week or more before a change set in; and in those unshed feathers carried over from the fall, it is quite reasonable to suppose that all connections with the body of the bird were as normal as in other feathers of a similar age. Before the change of color had begun, in December or January, in specimens examined carefully under the microscope, I could detect no presence of carrier pigment cells and found the calamus of each feather to be in the expected dried up condition. The change would thus seem to be confined to activity in the feathers alone.

In a brief summary of the principal points already discussed in connection with my bird we may conclude (1) that Aptosochromatism in my *Passerina cyanea* occurred beyond doubt, (2) that although present with severe feather loss it does not follow that the gain of color was directly responsible to it, as proved by careful examination of the newly acquired feathers, and (3) that although the feather loss was objectively independent of the Aptosochromatic change, it might subjectively be so, inasmuch as old and imperfect feathers were renewed for active and healthy ones, in which such a color change subsequently occurred.

The results quoted of Dr. Chadbourne and Mr. Maynard appear to me to be conclusive in themselves and require no further comment. Microscopically, the color change was not of as much prominence as might have been expected at first thought, but it will be recollected that blue and violet colored feathers depend, in a large measure, for their effects, upon involved objective superstructures, which act in combination with some underlying pigment or pigments. These pigments produce chemical objective effects, due to the absorption of all light rays not depending upon the characteristic color of the pigments.

Chemically and microscopically, the feathers of my *Passerina cyanea* appeared to contain two pigments, one, a diffused non-granular tawny colored stain, the other,

* Dr. Chadbourne has informed me, since the above was written, that of three confined male indigo birds observed by him, two changed color ptosochromatically and the third 'without any feather loss to speak of,' *i. e.*, aptosochromatically; and still later on, I was delighted to learn during a conversation with Mr. C. J. Maynard that he too had followed a male *Passerina* through its entire spring change of color. The bird involved, belonged to a friend of Mr. Maynard, who informed him that the bird had changed its colors during the season just passed without moulting its feathers. Determined to follow out the change exactly, Mr. Maynard examined the bird frequently the next season, throughout the entire time occupied by the change, and perfectly satisfied himself that it was totally unassisted by a moult or any considerable loss of feathers.

a granular blackish substance. The yellowish stain was confined to the transparent sheath of the barbs and to the barbules, while the granular matter varied in placement with the color of the feathers.

An unchanged, *i. e.*, fall-colored feather, examined in a fresh state, exhibited the following appearance under the microscope. The rhachis appeared, centrally, to be cellular in construction with an enveloping sheath thickly supplied with the black pigment matter, the granules arranged in an order suggestive of a streaming movement toward the tip of the feather. At the junction with each barb a small portion of the main system curved upward into the central portion of that member. Proximally this column ended in the modification of the rachis into barbs. The center of each barb of the colored parts of the feathers contained a prismatic column, resembling to my eye, a number of bodies set together so as to resemble the nodes and internodes of a bamboo cane. At the distal end of each barb these bodies tapered, and in many cases the extreme joints were separated from the main column. About the blue portions of the feathers, these columns were massed thickly with black matter, the portions giving the rusty effects being much less plentifully supplied, and surrounding this central column a transparent envelope of the yellowish stain was present. The barbules of the non-blue area were the color of this sheath but became well supplied with the dark pigment when the blue-producing area was met with, completely obscuring the presence of the stain. The tips of the prismatic columns showed a pale brownish orange color, but gradually as the microscope slide was passed across the stage the color became deeper until when one-half of the length of the barbs had passed before the objective, it appeared deep black.

The blue area of such a feather gave a good reaction for Zoomelanin (black), for by boiling the feather in KOH, 0.5% and then heating with chlorine, the dark matter was completely broken up, and the feather appeared colored as with the rusty-colored tip which was apparently unaltered by the test.

When compared to a feather wholly changed to blue by the Aptosochromatic process, a valuable suggestion is at once thrown out, as to the nature of the change of color. In a microscopic examination of such a feather it is noticed that the lower parts of the central barb colums were as in the bicolored-fall feathers excepting that the massing of the black appeared to be denser. The upper parts however which were deep orange brown before, varying to lighter tints as the tips of the barbs were approached, now appear thickly massed with the black also, and the yellowish barbules are likewise colored. The streaming movement of the color granules is now especially prominent in an actively changing feather, and it readily appears that the rhachis gives up a part of its matter to the barbs, which in turn supply it to the barbules. A positive change of pigment is manifested macroscopically, for a fall feather held to the light or crushed, remains yellowish in its yellow-colored parts, while a spring feather, appearing entirely blue, so treated, shows darkly, due to the addition of black pigment.

Undoubtedly the blue effects are produced by the prismatic column in coöperation with the dark involved pigment, the sheath enveloping these parts playing its part with the barbules in producing the fall-colored feather. A cross section of the blue-producing barb sustains this view. When placed under the microscope with all light obstructed, but that descending to the stage from above, the sheath became invisible but the central column showed up like phosphorus as a pale glimmering blue which became opalescent with the varying quan-

tities of light admitted. Unsectioned feathers so treated acted quite similarly, but the parts of the column appearing in the tips of the fall feathers, instead of betraying blue, showed gleaming white effects. Under transmitted light, as in ordinary examinations, the effects of the pigments alone were seen.

The massing of the granules of pigment begins evenly upon each vane, from the bottom of each barb and works towards the tips, the barbules being filled, from the tips first, as they are passed. The tips of the distal barbs usually were the last to completely undergo this change.

In both fall and spring feathers, the objective superstructure occupied the same relative position, being confined to the center of the barbs alone for almost their entire lengths.

Dr. Gadow, who has published results of his investigations upon the nature of the blue-producing structures in feathers, concludes that the production of blue is, in a measure, caused by the fine ridges of the prismatic columns, and thinks that the bodies of the columns and the transparent sheaths of the barbs may exert an appreciable influence. He adds in consequence "the production of blue therefore in a feather would be the result of a very complicated process."

As shown in my feathers, however, the blue appeared to be largely independent of the envelope of the barbs, yet this might concentrate light rays or so modify them that the consequent would be helpful. One thing appears certain, that to the presence of quantities of granular black-like matter is due no small share of the ultimate production of blue.

The causes of the differences in the shade of the blue feathers from violet to greenish, according to their position on the bird, appeared to be very slight microscopically, and I could detect no constant characters with the facilities at my disposal.

As to the causes of the activity necessary to produce a color change, we may only infer. As proved by dissection my bird was not undergoing any prominent sexual change, and the theory that the temperature of the atmosphere might be responsible would not be applicable to most cage birds which are kept in warm rooms. There can be no doubt, however, but that the fall change of plumage is one of protective tendency, and it is highly possible that until changed in the spring, the feathers, in a certain sense, are immature. In the case of the double yellow-headed parrot before mentioned, the color change was of a retrograde nature, but in the present example the process is synthetic rather than otherwise.

Numerous theories have been published which endeavor to account for the dichromatic fall change of many birds, but it would be irrelevant to discuss them here. One thing appears certain, that the process is deeply involved in the vital system of the organism. Professor Beddard cites an example noticed by Professor Weber of a chaffinch which was so colored that one-half of the bird was in the male plumage and the other portion in the female. Dissection proved the bird to be a Hermaphrodite, *i. e.*, the side sustaining the male plumage was found to contain a testicle, while the opposite portion of the body possessed an ovary, and Professor Beddard writes that this curious abnormality had been noticed before.

As no vascular connection appeared to be present in the perfect feathers of my bird, the change appears to be one of internal activity in the feathers themselves, and the simile before mentioned, of the autumn leaf, appears to be still more strongly consistent. The change is none the less vital, however, and ceases with the death of the organism. No tests delicate enough were applied to determine if new matter was formed directly in the feather. It appeared prob-

able that none was, but that the striking change depended upon the massing of the dark granular matter from the rhachis to the barbs and their appendages. The absence of definite data upon the chemistry of animal pigments makes remarks in a qualitative direction wholly undesirable.

To conclude our microscopic study, however, we may affirm: (1) that microscopically as well as macroscopically an appreciable Aptosochromatic change took place in the individual feathers of my *Passerina cyanea;* (2) that this change far from being analytical or retrograde was inclined to the nature of constructive synthesis, probably passive in nature; (3) that the change was definite as shown by comparison with the blue areas of unchanged feathers; (4) that it depended chiefly upon the gain of dark pigment in the vicinity of the prismatic column, and (5) that there was an appreciable difference in the amounts of blackish pigment supplied to the barbs and barbules, before and after the change.

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THE SOCIETY FOR PLANT MORPHOLOGY AND PHYSIOLOGY.

THE Society for Plant Morphology and Physiology held its third meeting, with the American Society of Naturalists and the Affiliated Scientific Societies, at Yale University, New Haven Conn., December 27th and 28th, with President J. M. Macfarlane in the chair. For the ensuing year the following officers were elected: *President,* D. P. Penhallow; *Vice-Presidents,* Roland Thaxter and Erwin F. Smith; *Secretary,* W. F. Ganong. The following new members were elected: Oakes Ames, J. M. Coulter, Carrie M. Derick, B. M. Duggar, A. W. Evans, M. A. Howe, L. R. Jones, Henry Kraemer, F. E. Lloyd, D. T. MacDougal, Conway MacMillan, G. T. Moore, Adeline F. Schively, Hermann von Schrenk, Julia W. Snow. The business transacted of most general interest was the appointment of a committee, consisting of W. G. Farlow, D. T. MacDougal and H. von Schrenk to consider ways of securing better reviews of current botanical literature. It was voted to communicate the views of the Society upon this subject to the editors of the *Botanisches Centralblatt.* The following papers were presented. The abstracts are furnished by the authors.

Geotropic Experiments: DR. G. E. STONE, Massachusetts Agricultural College.

This paper dealt with the question at which angle gravity acts most strongly on a geotropically sensitive organism. The results were obtained by the use of grass nodes and in one or two instances the roots of *Vicia faba* were used. Three methods of experimenting touching upon the solution of this problem were described.

The first series give the results of dynamometer experiments in which the power of growth shown by different nodes placed at different angles was illustrated.

The second method consisted of taking the average of a large number of cut plants grown in moist sand and placed at different angles.

The third method showed the results of experiments due to the after effect of stimulation.

The results of all these experiments were similar and may be partly summarized as follows:

The horizontal position is the position of greatest geotropic excitability. This is shown by the increased amount of weight nodes will lift in this position, the amount of growth they display and the after effect reactions.

The relationship existing between nodes at oblique angles and those in a horizontal position is one which is proportional to the cosines of their angles. This also holds

good when the nodes were placed at angles below the horizontal position. Young nodes show a definite response to geotropic stimulus when exposed for so short a period as 30 seconds. The latent period is about 30 minutes long.

On the embryo sac of Saururus cernuus: Dr. D. S. Johnson, Johns Hopkins University.

The development of the bract and flower is essentially like that found in the Piperaceæ by Schmitz. In the material studied there were two ovules to each carpel, of which only one, the upper one, matured to a seed. There are two integuments as in most of the Piperaceæ and both grow up to form the micropyle. The primary archesporial cell is single and hypodermal in position. It divides to a definite archesporial cell and an upper tapetal cell. The former then divides to three potential macrospores, of which the lower only becomes functional. This grows and the nucleus divides in the usual way to form a seven nucleate embryo sac. The embryo sac soon widens at the base to become flask-shaped, with the egg apparatus at the upper end of the neck and one endosperm nucleus in the neck and probably a second one in the body of the flask. The antipodals soon become indistinguishable, probably degenerating. Endosperm cells are formed in the neck of the flask before any change can be seen in the egg itself. In the ripe seed the endosperm is found to have swelled laterally using up only the tip of the large nucellus while the lower or body end of the embryo sac is still without endosperm cells or nuclei. The features of the development of *Saururus* thus far studied give no conclusive evidence of its primitive character.

Upon the best way of securing a good review of Current Botanical Literature: Professor W. G. Farlow, Harvard University.

Professor Farlow's address upon this subject appeared in synopsis in the *Botanical Gazette* for January. He pointed out that this is a question distinct from that of a card catalogue, international or other, of literature. None of the existent journals fully meets the needs of botanists for prompt descriptive synoptic reviews of current literature. The *Botanisches Centralblatt* comes the nearest to the needed journal, and if its reviews were more prompt, and, in many cases, of a more descriptive character, and if it did not take up with original articles so much space which ought to be given to reviews, thus forcing many of the latter into Beihefte for which an additional subscription must be paid, it would be much more satisfactory. The discussion following this paper showed that similar opinions are held generally by botanists in this country, and steps were taken to communicate the Society's opinions upon the subject to the editors of the *Centralblatt*.

Fasciation in the Sweet Potato: Mr. Henry S. Conard, University of Pennsylvania. (By invitation.)

The author stated that typical fasciations ½ in. to 3 in. broad are common in the sweet potato. The internal structure of these differs from that of normal stems only in the shape of the cross-section. On poor soil 12% of the stems are abnormal, on good soil 18% ; counting apices only, 20% are abnormal on poor soil, 54% on good soil. Of the abnormal stems ½% to 1% are ring-fasciated. Such stems enlarge gradually from the base, but maintain a round cross-section. They become hollow within, the cavity opening to the air at the growing apex. The apex has a wavy margin; or the tube may be split above, and may give rise to plain fasciation. The tubular portion may be as much as 2 or 3 ft. long by ½ to ¾ in. in diameter. The cavity has on its walls acropetally developed leaves, and adventitious roots; the latter become func-

tional when cuttings of the ring-fasciation are planted. The transverse section shows two bundle-systems, an outer normal one, and an inner surrounding the cavity with its normal phloem facing the cavity. The hollow is bounded by epidermis and cortex. Internal phloem (bicollateral structure) is a marked feature of both internal and external bundle-systems of the ring-fasciated stem, as it is of the normal stem. The two bundle-systems of the anomalous specimens are entirely separate, but merge into common ring-shaped apical meristem. Toward the root the cavity tapers to a close, leaving a ring of bundles; these dwindle away one by one, the phloem elements persisting longest. The writer's observations agree essentially with similar ones of de Vries and Nessler; he also considers Masters' 'tubular stem of Sempervivum' and Qualch's 'fasciated sweet-pea' as probable cases of ring-fasciation.

Leaf Scorch of the Sugar Beet: MR. F. C. STEWART, New York Experiment Station.

During August, 1899, some fields of sugar beets in Central New York were severely injured by sudden scorching of the foliage due to excessive transpiration. The foliage, wholly or in part, turned black and died. A majority of the affected plants recovered and made a second growth, but some were killed outright. The dead roots showed internal browning around the circumference and dark colored, raised, somewhat V-shaped areas on the surface. Later in the season the raised surface areas became affected with dry rot and the dead tissue was eaten away by millipedes, leaving shallow cavities resembling scab spots. It has been proved by an inoculation experiment that the internal browning is not caused by parasitic organisms. All affected beets were low in sugar content.

This paper has been published in New York Agricultural Experiment Station Bulletin, No. 162.

Distribution of red color in vegetative parts in the New England flora: MISS F. GRACE SMITH, Smith College. (By invitation.)

The author called attention to the various and more or less conflicting views as to the meaning of the presence of red color in vegetative parts of plants. In order to test these theories, observations were made to determine how many plants in the New England flora show the red color. Careful account was taken of the exact structural part in which the color occurs, and of the relation of the position of the color to external conditions of light, dryness, etc. The results were reduced to percentages and showed that none of the existent theories will explain the distribution of the color in our vegetation. The conclusion is drawn that either the red color must have several different reasons for being, or else it has some significance to which we as yet have no clue. The studies are to be continued.

On the morphology of certain plants from the Devonian of Europe and America: PROFESSOR D. P. PENHALLOW, McGill University.

The author drew attention in particular to two types of plants which had been the subject of investigation for a number of years.

In 1831 Fleming described certain interesting and peculiar fossils from the Devonian of Scotland under the name of *Parka decipiens*, and subsequently Hugh Miller gave them much consideration in his Old Red Sandstone. Since that time they have excited the interest of paleontologists at various times, but always with reference to the eggs or spawn of some animal form of life. In 1891 they were first clearly proved to be of plant origin by Dawson and Penhallow, who, however, were unable to

clearly establish their relations to existing types. So far as investigations have proceeded, we can now recognize a sporocarp enclosing both macro- and micro-sporangia, from which may be obtained the corresponding spores with dimensions of 40 and 15 microns respectively. Prothalli in various stages of development have also been obtained. There is presumptive evidence that the sporocarp may have been stalked as in Marsilia, but the character of the contents points more to an affinity with Pilularia. The association of creeping rhizomes strengthens the supposition that they were aquatics of the general habit of growth of Marsilia and Pilularia, while the recently discovered occurrence of leaves of the general type of Marsilia would seem to add to the validity of this hypothesis. Although closely and constantly associated in the same beds, and even on the same slab, the unfortunate separation of members will not permit final conclusions as to the nature of this plant at the present time.

In 1885, the late Sir William Dawson discovered in the Devonian sandstones of Gaspé, the remains of a gigantic alga to which he subsequently gave the name of *Nematophyton*. This genus has since been found to have a very wide distribution, being found in Great Britain, Germany, and various parts of the United States and Canada as far west as Ohio. At present eight species are distinguished, but it is highly probable that with a more complete knowledge of the plant, this number will be greatly reduced, while the number of genera may have to be increased.

They were plants of arborescent habit, attaining a diameter of two feet or more and a height which was undoubtedly great, but which can only be conjectured from the diameter and the fact that the largest specimen so far recovered from the Hamilton Group of New York, although incomplete, had a length of 24 feet. No foliage or fruit has yet been obtained, and our only guide to relationship is through the character of the internal structure. A pseudo-exogenous structure, as in many Laminarieæ, is conspicuous. Isodiametric, radial or irregularly branching openings occur in the various species, and in these areas or in their immediate neighborhood, the larger cellular elements branch more or less freely into small hyphæ which form an intercellular plexus. The principal elements of structure consist of exceedingly long, non-septate, thick-walled, tubular cells which traverse the stem in the direction of the longitudinal axis—though to a minor extent transversely—and interlace freely to form a medulla as in the larger forms of the Laminarieæ. No surface markings of the walls have been observed, but in one species (*N. Ortoni*) the cells show local enlargements closely resembling the trumpet hyphæ of *Macrocystis*.

One of the most instructive features of these plants is to be found in certain structural modifications which they exhibit. In working up material from various sources, it was discovered that many specimens exhibited certain transitional forms from normal structure to what was described many years since by the late Sir William Dawson under the name of *Celluloxylon primævum*. A closer study revealed the fact that this alteration was incident to the crystallization of the infiltrated silica and the operation of decay, whereby the carbon particles of the original cell walls were redistributed upon the surfaces of the crystals in such a manner as to produce the general effect of a coarse cellular structure.

Notes on the morphology and reproduction of Chlorocystis Cohnii: Mr. G. T. Moore, Dartmouth College. (By invitation.)

The structure and development of this unicellular alga growing on Entromorpha was described and several points differing

from those previously recorded were referred to. It was shown that the habit of the plant is not that of a universal endophyte but is quite as often merely epiphytic. The character of the chromatophore was found to vary from the 'one-sided' arrangement, considered by Reinhard as typical, to a condition where the cell wall was completely lined by the color body. Two sizes of zoöspores, formed by successive divisions, were described, but no suggestion of conjugation could be observed and it seems probable that nothing of the kind occurs. In the material examined the discharge of zoöspores was found to take place through a circular opening of considerable size instead of through a tubular neck as recorded by European writers on this subject. The paper will be published in full later.

The roots and mycorhizal adaptations of the Monotropaceæ: PROFESSOR D. T. MACDOUGAL, New York Botanical Garden, and PROFESSOR F. E. LLOYD, Teachers College. (By invitation.)

The Monotropaceæ have been under investigation with respect to their mycorhizæ since 1840. Of the genus *Monotropa*, *M. Hypopitys* only has received consideration, and all references made in text-books hitherto have referred to this plant. *Monotropa uniflora* has now been added to the list. In this plant the roots are found to be entirely invested by the mycelium of the fungal symbiont, even over the cap. The latter is 2–4 cells in thickness but arises from a calyptro-dermatogenic layer. The plerome and periblem arise from a common initial tissue and may not be distinguished for some distance back from the apex. The stele and cortex arising therefrom are not distinctly delimited, there being no well marked endodermis. The stele is very irregular in structure. The hyphæ of the fungus gain access to the epidermal cells at about the backward limit of the root-cap, but never enter these. The short haustorial branches which gain entrance enlarge into more or less irregular vesicles, and these come partly to surround more or less the nucleus which, however, does not become hyperchromatic, but maintains its normal condition for a long time. The epidermal cells ultimately become separated from each other by the hyphæ which penetrate between them, but not between the cells of the cortex, or only very rarely. The vesicles sometimes produce spore-like bodies and are believed to be reproductive. A most interesting gradation from the arrangement of the mycorhizas in the nearly related *Pyrolas* is shown. The branching of the root in *M. uniflora* is exogenous, this result not according with that of Kamiensky on *Hypoitys*.

The general statements which may be made concerning all the Montotropaceæ now studied with reference to their mycorhizas are as follows:

1. The shoots are free from chlorophyll, and have no stomata except in the case of *Pterospora*.

2. The customary relations in size of the root and shoot are lost.

3. The stele is much reduced in both the shoot and root, and shows perforated vessels and companion cells only.

4. The fungus sheathing the root encloses the tip completely, at least at certain seasons, and penetrates the epidermal cells in all of the genera examined. Vesicles, sporrangioids or organs of interchange are formed in the epidermal cells. The relation between the fungus and the seed plant is a pure symbiosis and the latter does not act as a fungus trap in accordance with Frank's theory.

5. The root cap is one to many layered, and with the epidermis is derived from a dermato-calyptrogenic layer. The periblem and plerome are not distinguishable in form or content.

The structure and reproduction of Compsopogon: Dr. Roland Thaxter, Harvard University.

The author gave some account of the distribution of *Compsopogon* in Florida, mentioning the localities where it had been found by himself and others, in fresh as well as partly tidal waters. Its general structure was described, attention being called to the fact that the older filaments may possess a cortex of from two to four layers of cells. Details of cell structure and the normal reproduction by aplanospores were illustrated, as well as the formation of small aplanospores developed from sorus-like groups of superficial cells.

On some diseases of New England Coniferæ: Dr. Hermann von Schrenk, Shaw School of Botany. (By invitation.)

The coniferous woods of this region are being destroyed by the mycelia of a number of fungi, chief among which are, *Polyporus tenella*, *Polyporus Schweinitzii*, *Polyporus piceinus*, *Polyporus pinicola*, *Polyporus sulfureus* and *Agaricus melleus*. The mycelia bring about characteristic changes in the wood either by destroying the lignin and leaving pure cellulose, or by transforming the wood into a brown brittle substance. The changes are caused by enzymes one of which was obtained from *Polyporus tenella*, capable of destroying the substance hadromal, leaving pure cellulose. This enzyme is not diastase. The extent to which decomposition is carried on is apparently determined by decomposition products such as humus compounds, citric and succinic acids and others. These stop the action of the enzyme at a certain point. The enzyme transforms the wood in great quantities; many pounds of pure cellulose are often found in one place. Masses of cellulose and some hadromal were exhibited. The mycelia of these fungi live both as parasites and saprophytes, some entirely in the ground. The sporophores of many excrete manitose in quantity at the time when the spores are ripe. This may aid in the dissemination by attracting bark beetles. Six forms of wood destruction were described, and specimens and photographs of the same were shown.

Vegetative reproduction and multiplication in Erythronium: Mr. Frederick H. Blodgett, New York Experiment Station. (By invitation.)

The presence of underground runners in *Erythronium americanum* has been recognized for some time. The development of the runners, and especially of the bulbs which are formed from their terminal buds was published in 1895. In the present article the author describes the common origin within the bulb of these runners and of the annual bulbs of the mature individuals, as axillary buds between the base of the stem and the inner bulb scale. The development of the first bulb from the seedling is also described. It is characterized by containing the plumule instead of the normal foliage leaf and remains latent for a year. From one bulb, thus developed from a seed, the life cycle is shown to occupy not less than four years, probably six or seven years in most instances. During this interval runners are given off each year, usually three annually, resulting in a bed in five years of from six to nine plants for each seed, or ninety plants if ten seeds of a single fruit should survive all steps of the cycle.

Current problems in Plant Cytology: Professor J. M. Macfarlane. (Presidential Address.)

This address is expected to appear in abstract in Science.

The structure of starch grains: Dr. Henry Kraemer, Philadelphia College of Pharmacy. (By invitation.)

In the different text-books starch grains

are represented as having either a light-colored point of growth (nucleus or hilum) with alternate dark and light lamellæ as figured in Sach's works or having a dark point of growth and alternate light and dark lamellæ as illustrated by Strasburger. The appearance of the grain as given by these and other writers is dependent upon the view of the grain by the observer. In focusing upon the top of the successive lamellæ of the grain, one observes the view as given by Strasburger; whereas if one brings the focus to the base of the lamellæ the appearance of the grain as given by Sachs is observed. If we focus upon the top of the various lamellæ of the potato starch grain, we find not only grayish and light colored lamellæ alternating with each other, but we find in the center of the grayish colored zone at the point of growth a much darker area. A layer having the same appearance as this dark area is also observed on the outside of one or more of the grayish lamellæ. The grayish and dark colored layers, just referred to, have a reddish appearance, the dark colored layers being slightly bluish also.

Upon treating the starch grains with an aqueous solution of an aniline stain such as safranin, gentian violet, etc., the stain is apparently taken up by the darker lamellæ, *i. e.*, when focusing upon the top of the layers or lamellæ. On the other hand on treating the grains with chromic acid, calcium nitrate, etc., or with water at different temperatures for varying periods of time, the characteristic sphero-crystalloidal structure is brought out in the light lamellæ, *i. e.*, when focusing on top of the layers or lamellæ.

We find further that the layers which do not take up the aniline stains become blue with iodine, whereas the alternating layers are not at all affected apparently by this reagent. It appears that the layers not affected by iodine, but stained by the aniline colors, are rich in colloidal substances but poor in crystalloidal materials. These correspond to the cellulose layers of Nägeli, the farinose layers of von Mohl and the β-amylose layers of Meyer. The layers stained blue by iodine and not stained by the aqueous aniline solutions, are poor in colloidal substances but rich in crystalloidal materials, these corresponding to the granulose layers of Nägeli and von Mohl and to the α-amylose layers of Meyer.

The toxic action of a series of Sodium Salts: DR. RODNEY H. TRUE, Cambridge, Mass. (By invitation.)

From experimental results worked out by Drs. Kahlenberg and True, the latter formulated the results presented under this title.

After studying the toxic action exerted on roots of *Lupinus albus* by a series of acids and by their Na salts, in view of the ionization of these compounds, an analysis of their toxic action has been made into the partial-toxicities due respectively to H ions, anions and ionized molecules. In inorganic acids of the type of HCl, the action of the anions is so slight, relative to that of H ions, as to be thrown into insignificance. In the organic acids where ionization is usually much less advanced, the predominance of the H effect is less marked. In formic, salicylic, ortho-nitrobenzoic and protocatechinic acids, the H ions exceed in effectiveness all other factors. In propionic, butyric, gallic, cinnamic and hippuric acids the action of the unionized molecules predominates over all other factors. In the sodium salts where a greater degree of ionization exists, a greatly diminished toxic action is seen, due chiefly, apparently, to the action of the anions. In carbolic acid, having its hydrogen in the hydroxyl (—OH) form as contrasted with the above organic acids in which it exists as carboxyl (—COOH) hydrogen, ionization is at a

minimum and the toxic action is due solely to unionized molecules.

Further notes on the Embryology of the Rubiaceæ: Professor F. E. Lloyd, Teachers College. (By invitation.)

The Rubiaceæ reported upon up till now include the *Stellatæ*. The present paper deals with *Diodia virginiana, D. teres, Richardsonia pilosa* and *Cephalanthus occidentalis*. All are similar morphologically as to the topography of the ovule. They vary, however, in the relative rapidity of development of the basal partition and of the nucelli. *Cephalanthus* possesses the most rapidly developing basal partition with the result that the ovule is rotated so that the funicle is inserted in the top of the ovary and the embryo sac is inverted. All possess an outgrowth of the funicle, resembling a second integument, which in *Richardsonia* and *Diodia* becomes loaded with raphides and is believed to have a nutritive rôle. The 'spongy' funicular appendage in *Cephalanthus* is shown to be the homolog of that in *Diodia* and *Richardsonia*, but the nutritive function is here less probable. The funicle in *Diodia* and *Richardsonia* is surrounded by a collar of peculiar epithelial cells which in *Richardsonia* are the path of the pollen tubes; the latter never travel freely in the ovarian cavity. The archesporium consists of but one cell which gives rise to four megaspores. One of these is the embro-sac mother cell. The embryo-sac is of the elongated type with the usual number of cells. One of the antipodal cells is rather long in *Diodia*, but the three are of equal size in *Richardsonia* and *Cephalanthus*. *Diodia virginiana*, however, has four antipodal cells from the division of the long cell after the small ones are cut off. The embryo possesses a short cylindrical suspensor from which the haustoria characteristic of the *Stellatæ* are absent. There is, however, evidence of a nutritive function. The embryo develops at first very slowly, in contrast with its rapid early development in the *Stellatæ*, a fact which is correlated with the absence of haustoria. The endosperm at first is parietal.

The stimuli that cause the so-called 'peg' or 'heel' on Cucurbita seedlings: Dr. J. B. Pollock, University of Michigan.

No abstract of this paper is available.

On the prothallus of Taxodium distichum Richard: Mr. W. C. Coker, Johns Hopkins University. (By invitation.)

The archesporium consists of a group of cells near the base of the nucellus. One of its central cells enlarges at the expense of those around it and becomes the embryo-sac. The archegonia arise at the upper end in a group and soon come to lie in a depression, which is at first covered over by the wall of the embryo-sac. There are four neck cells, and a ventral canal cell is cut off. Soon after the formation of its cell-walls the endosperm cells become multinucleate by amitotic division.

The pollen sprouts soon after pollination (which in Baltimore takes place about the middle of March) and reaches the embryo-sac before a solid endosperm has been formed. The end in contact with the sac now contains three nuclei, not greatly differing in size, one of which is the body cell nucleus. This increases in size, surrounds itself with a dense mass of cytoplasm, and just before fertilization divides into two male pro-nuclei each with its mass of cytoplasm. Several pollen tubes may push their much enlarged ends into the depression in the embryo-sac. The male and female pro-nuclei come in contact near the upper end of the oösphere and move towards its base. Fusion is complete about the time the base is reached. The young pro-embryo consists of four tiers of four cells each, the upper tier lying free in the archegonium.

A new type of branching in the leafy Hepaticæ: DR. A. W. EVANS, Yale University. (By invitation.)

According to Leitgeb, who made a thorough study of the various kinds of branching exhibited by this group of plants, his so-called 'Endverzweigung' or 'terminal branching' always occurs in the ventral half of one of the lateral segments cut off from the apical cell. In the curious *Mastigobryum integrifolium* Aust. of the Hawaiian Islands, this same kind of branching occurs in both lateral and ventral segments, showing that it is much less restricted than Leitgeb supposed. In this case the ventral branches, apparently from their place of origin, are specialized as flagella.

The geotropism of split stems: DR. E. B. COPELAND, University of West Virginia. (By invitation.)

Numerous experiments show that if a stem be split into two equal halves and then placed in a horizontal position, the lower half is stimulated to grow more rapidly than the upper. As compared with an erect half stem, the upper horizontal half (that with the split surface downward) has its growth depressed, while the lower horizontal half has its growth considerably more accelerated. This is what happens in an uninjured prostrate stem in the execution of a response to the geotropic stimulus, except that the difference in growth between the halves is much greater when they are separated.

It has been held that in the geotropism of stems there must be at least a transverse transmission of the 'sensation,' by which the halves can compare their positions; but in view of the behavior of the isolated half-stems this argument is obviously invalid.

Some variations and correlations in the leaves of trees: MISS HARRIET B. WINSOR, Springfield, Mass., and DR. W. F. GANONG, Smith College.

This paper, offered as an illustration of method, represented the results of an attempt to apply statistical methods to the study of an ecological problem, namely the factors determining the length of petioles and the size and shape of the leaf. The studies are to be continued.

Perennation in the stem of Lycopodium alopecuroides: DR. J. M. MACFARLANE, University of Pennsylvania.

The author pointed out that in this species the branch extremities begin to dip into the ground in autumn by what has been proved to be geotropic growth. This portion becomes colorless, greatly loaded with starch, and bears leaves that become specially modified from those of the normal green type. When the perennating part is fully developed it is a hook-like depression which lies dormant until the succeeding spring. By succeeding apogeotropic growth it again reaches the surface and assumes its usual development.

Of special interest is the fact that the above peculiarity is being gradually acquired, for while a large proportion of plants show it, a few that may grow protected in loose sphagnum or under water only show it to a feeble extent.

The phytoëcology of the Bay of Fundy salt marshes: DR. W. F. GANONG, Smith College.

At the head of the Bay of Fundy occur great salt marshes which bear a remarkably varied vegetation, showing transitions from the usual salt marsh to fresh-water bogs. The reclamation of the marshes for cultivation brings in another element, and all transitions may be found between the salt and the reclaimed marsh. These transitions of conditions afford an unusual opportunity to investigate some phases of the dynamical relationships of plants to their surroundings, and the author has attempted to work out this problem in ecology. The

physical conditions of the marshes are discussed, the flora described and the segregation of the vegetation into formations illustrated. The physiological and structural peculiarities of the leading plants of each formation are then described. Finally an attempt is made to formulate the results into a series of principles of general application.

Complications in Citrus hybridization caused by polyembryony: Mr. H. J. Webber, Department of Agriculture.

During the last five years the writer has made about a thousand hybrids of the various species of *Citrus* commonly cultivated, and has had under his care and observation about an equal number made by Mr. W. T. Swingle. Last spring a careful comparison of the foliage characters of these hybrids led to the interesting observation that where two or three seedlings were developed from a single seed, as was frequently the case, they not infrequently showed marked foliage differences. The fact that the common orange and many other species of the genus *Citrus* are commonly polyembryonic is well known. A single seed of the common orange has been known to produce as high as thirteen different seedlings, although it is seldom that more than three of the embryos are capable of development. Strasburger, in his critical study of the polyembryony of this group, found that the embryos, other than that developed from the fecundated egg cell, are derived from certain cells of the nucellus lying near the embryo sac wall which become specialized, grow and divide rapidly, and form a tissue mass which pushes out into the embryo sac and forms an embryo similar to that formed in the normal way from the egg cell. The embryos formed in this way Strasburger called adventive embryos. If we correctly understand the action of fertilization it is clear that in hybridizing fruits of this sort only those embryos developed from the egg cell proper, as a result of the fecundation, would show an indication of the hybridization. The adventive embryos developed directly from the mother tissue we should not expect to show any of the characters of the male parent. This was the conclusion reached by Mr. Swingle and the writer jointly in discussing the matter several years ago, and the development of the hybrids has now shown this to be the case. In several instances of hybrids of *Citrus aurantium*, which has unifoliolate leaves, with *C. trifoliata*, which has trifoliolate leaves, where the former was used as the female parent, two and three seedlings have been produced from the same seed, one of which had trifoliolate leaves and the others strictly unifoliolate leaves exactly like those of the mother parent. In such cases it is evident that the trifoliolate seedling inherits this character from the male parent and that the embryo from which it grew was developed from the egg cell proper. The other seedlings in such cases, having unifoliolate leaves like the mother parent, are doubtless developed from the so-called adventive embryos. The writer has also observed the same phenomenon in the reciprocal hybrids, *Citrus trifoliata* ♀ × *C. aurantium* ♂, and in the hybrids of *Citrus nobilis* ♀ × *C. aurantium* ♂. The observations have been sufficiently extended so that we may be certain of the common occurrence of the phenomenon in citrous hybridization.

In hybridizing citrous fruits to secure improved sorts this fact unfortunately introduces a serious complication. The majority of citrous hybrids, in almost every case resemble the female parent in foliage characters, and it will thus be seen that until they fruit it will be impossible to determine whether they are true hybrids, showing a preponderating influence of the

female parent, or simply false hybrids developed from adventive embryos. It is thus probable, or we may say certain that in such work many seedlings will have to be grown and fruited which are from adventive embryos and are not true hybrids. These of course cannot be expected to give rise to valuable new varieties, and growing them will greatly increase the trouble and expense.

W. F. GANONG,
Secretary.

SCIENTIFIC BOOKS.

Insects, their Structure and Life. By GEORGE H. CARPENTER, B.Sc. London, J. M. Dent & Co. 1899. Pp. 404, figs. 183.

There is need of a modern elementary text-book of entomology covering all the important phases of the study of insects. Comstock's Manual in its present form is chiefly devoted to the systematic and ecologic phases, while Packard's new text-book is given up exclusively to the morphological, physiological and developmental phases of insect biology. Mr. Carpenter's book is an attempt to supply the need. Its six chapters treat respectively of the Form of Insects (anatomy and, very slightly, of physiology), the Life-history of Insects (embryonic and post-embryonic development), the Classification of Insects, the Orders of Insects (these two chapters including the classification of insects as far as families, and brief mention of the habits with families as units), Insects and their Surroundings (ecology) and the Pedigree of Insects (phylogeny). These subjects include all the principle phases of the general biologic study of a group of animals, and in this respect the book is wisely planned to meet a real need. For the most part this presentation of the elementary facts of the 'structure and life of insects' will meet with the approval of teachers of entomology. The selection from the mass of material constituting the science of entomology of the little that can be included in an octavo volume of 400 pages, is a matter requiring a large knowledge of insects and a discriminating and clear pedagogic insight. The author (an active naturalist of Dublin) has a good knowledge of entomology, a discriminating perception of the relative importance of facts, and a clear and simple style.

In undertaking to write an elementary general text-book of entomology, the most difficult task is that of the satisfactory treatment of the systematic phase. The enormous number of insect species precludes the use in such a book of classificatory units smaller than families, and indeed renders the adoption of the family unit unsatisfactory. It is this part of the book, the chapter, Orders of Insects, devoted to the systematic consideration of insects, which is the least satisfactory part of it. To treat systematically the whole class of insects, using families as units, in a few more than one hundred octavo pages, and to impart to this treatment any real interest or life, or, one is forced to say, real value, is too nearly impossible to be expected from even the capable author of this book. He adopts a 15-order classification and races through these orders, leaping family barriers three or four to the page. To be sure, certain mechanical assistance, a resorting to small type, is offered to give each family a chance to have its habits told in two lines instead of one, but that hardly betters matters. There is simply no space for what is the absolute minimum of treatment necessary to make such a syllabus or synopsis worth anything more than a list.

It is with relief, therefore, that we leave this distressful attempt to do the impossible, to examine the following interesting and admirable chapter on 'Insects and their Surroundings,' where those general relations of insects to their surroundings, distribution, parasitism, protective resemblances and mimicry, social and communal life and other phases of insect ecology, are presented. Similarly good are the chapters on embryonic and post-embryonic development. Much of the matter of these chapters has never before been given in a small text-book of entomology, and that is the special value of this book. The illustrations are, while mostly good in themselves, often not very apposite to the context. Very few new figures have been made for the book. The excellent blocks (more familiar to American entomologists than they probably are to English students) of the Divi-

sion of Entomology of the U. S. Dept. of Agriculture have been drawn on heavily for the book's illustration. One hundred and one out of the one hundred and eighty-three figures are from this source.

A brief but good bibliography is appended and there is an index. The work of printers and binders has been tastefully done. Altogether the book is a useful one, and one that can be recommended to beginning students of insects.

VERNON L. KELLOGG.

STANFORD UNIVERSITY, CALIF.

The Strength of Materials. By J. A. EWING, Professor of Mechanism and Applied Mechanics in the University of Cambridge. Cambridge, The University Press. 1899. Octavo. Pp. 246.

It will be news to many Americans to learn that instruction in the subject of the strength of materials is now given at the University of Cambridge and that laboratory work in testing is done there. The mathematical theory of elasticity has long received attention at the universities of England and Scotland, as shown by the works of Todhunter and Pearson, of Love, and of Thompson and Tait; this theory and these volumes have, however, added little to the practical knowledge of the properties of materials and have not influenced engineering constructions. At last, after many years of waiting, there comés from Cambridge a book which recognizes the fact that observation and experiment are essentially necessary, and which sets forth the fundamental principles and facts in a manner likely to be of much value to the engineering students and civil engineers of Great Britain.

In its theoretical discussions the book covers about the same ground as that given in American engineering colleges, but there are few numerical exercises and no problems for solution by the student. That such problems are necessary is, however, recognized by the author in his preface which states that the volume is a lecture room treatment of the subject and should be supplemented by laboratory work and computations. The theory is not given isolated from experience, but methods of testing are explained in an interesting manner, and the principal conclusions of the authorities in all countries are noted. Processes of manufacture which influence strength and ductility also receive some attention. The theory of beams, columns and shafts is presented clearly and concisely, and the subject of stresses in trusses and arches is briefly treated. The author has succeeded admirably in putting much sound doctrine and practical information into a limited space. The notation and terminology leans toward that of the mathematical theory of elasticity, but here and there the author breaks away from that bondage and uses the notation of engineering literature. In short this happy wedlock of theory and practice is one upon which the University of Cambridge should be congratulated. No book has appeared in England in recent years which so fully corresponds to the American ideal of a text-book for sound and successful education.

M. M.

GENERAL.

PART V. of the 19th Annual Report, and accompanying Atlas, consists of a collection of papers and reports of the U. S. Geological Survey descriptive of the forests of the West, especially of certain of the forest reserves created by Executive Order of February 22, 1896, prominent among which are the Black Hills, Bighorn, Teton, Yellowstone Park, Priest River, Bitterroot and Washington Forest Reserves. Copies may be secured through United States Senators and Representatives, or by application to the Survey.

THE Liverpool School of Tropical Medicine has just issued a memoir, or small book, containing 'Instructions for the Prevention of Malarial Fever,' for the use of residents in malarious places.

BOOKS RECEIVED.

The Ore Deposits of the United States and Canada. JAMES FURMAN KEMP. New York and London, The Scientific Publishing Co. 1900. Pp. xiv + 481.

Annals of the Astronomical Observatory of Harvard College. EDWARD C. PICKERING, Director. *Visual Observations of the Moon and Planets.* WILLIAM H. PICKERING. Vol. XXXII. Part II. Pp. iv + 117–317. Plates viii–xiv. *Miscellaneous Researches.* Vol. XXXIII. Pp. 287. *Observations made at the Blue Hill Meteorological Observatory.* A. LAWRENCE ROTCH. Pp. 131–280. Cambridge, 1900.

Cyclopedia of American Horticulture. L. H. BAILEY assisted by WILHELM MILLER and many expert Cultivators and Botanists. In 4 volumes. New York, The Macmillan Company. 1900. Vol. I. A.–D. Pp. xxii + 509. $5.00.

SCIENTIFIC JOURNALS AND ARTICLES.

American Chemical Journal, January, 1900. 'On the Molecular Rearrangement of *o*-Aminophenylethyl Carbonate to *o*-Oxyphenylurethane,' by J. H. Ransom. 'Diazocaffeine,' by M. Gomberg. 'The Action of Ethyl Iodide on Tartaric Ester and Sodium Ethylate,' by J. E. Bucher. This number also contains a note on 'Improvements in the Manufacture of Sulphuric Acid.'

February, 1900. 'On Some Abnormal Freezing-point Lowerings produced by Chlorides and Bromides of the Alkaline Earths,' by H. C. Jones and V. J. Chambers. 'The Preparation of Pure Tellurium,' by J. F. Norris, H. Fay and D. W. Edgerly. The authors first prepared pure basic nitrate and then prepared the tellurium from this. 'The Reduction of Selenium Dioxide by Sodium Thiosulphate,' by J. F. Norris and H. Fay. 'Action of Picryl Chloride on Pyrocatechin in Presence of Alkalies,' by H. W. Hillyer. 'Camphoric Acid,' by W. A. Noyes. 'On the Rearrangement of Imido-esters,' by H. L. Wheeler. 'The Double Halides of Antimony with Aniline and the Toluidines,' by H. H. Higbee. 'On the Rancidity of Fats,' by I. Nagel. A note on 'The Wax of the Bacillariaceæ and its Relation to Petroleum.' J. ELLIOTT GILPIN.

The American Naturalist for January is a little late in making its appearance, but its contents may excuse the delay. The first article, by Henry Fairfield Osborn, is a most important contribution to the subject of 'Intercentra and Hypapophyses in the Cervical Region of Mosasaurs, Lizards and Sphenodon,' and is well illustrated. Ales Hrdlicka describes in some detail the 'Arrangement and Preservation of Large Collections of Human Bones for Purposes of Investigation,' and A. D. Mead has an article 'On the Correlation between Growth and Food Supply in Starfish,' in which he shows that starfishes of the same age may vary greatly in size. E. H. Eaton discusses 'The Zoology of the Horn Expedition' to Central Australia, and its bearing on the faunal affinities and geologic changes of Australia, and Henry Fairfield Osborn notes 'A Glacial Pot-Hole in the Hudson River Shales near Catskill, N. Y.' John H. Lovell in 'The Visitors of the Caprifoliaceæ,' describes the structural peculiarities of various genera and species of the honeysuckle family and notes the species of insects which he has observed to visit them, supplementing his notes by the observations of others. There is a large number of reviews, particularly of zoological papers.

THE *Journal of the Boston Society of Medical Science* for January opens with an article by Harold C. Ernst on 'Instruction in Bacteriology in the Medical Schools of America and Europe,' giving an analysis of the replies received from ninety-eight institutions to a circular letter of inquiry. The remainder of the number is devoted to abstracts of papers presented at the meeting of the American Public Health Association, Section of Bacteriology and Chemistry, held October 30, 1899.

SOCIETIES AND ACADEMIES.

SCIENCE CLUB OF THE UNIVERSITY OF WISCONSIN.

AT a special meeting of the Science Club of the University of Wisconsin, on the evening of January 24th, Mr. T. C. Chamberlin, of the University of Chicago, addressed the Club on 'Some recent studies of fundamental problems in geology.' Mr. Chamberlin was for five years president of the University of Wisconsin, and a large audience gathered to greet him and to listen to his admirable presentation of an exceedingly difficult subject. By reason of the fact that many of his hearers were not specialists in science, the address was semi-popular, and by special request of the president of the Club, Mr. C. R. Van Hise, it treated particularly of Mr. Chamberlin's well-known studies in this field. These studies have already engaged his attention for a number of years and are not yet completed. Some of the most important conclusions reached by Mr. Chamberlin were given to his audience as he explained 'in confidence to his friends,' as they are not

yet in form for publication. The problems discussed—the origin of the solar system, the source of the earth's heat, the earth's physiognomy, its atmosphere, etc., had all been involved in an attempt to frame an adequate working hypothesis to explain the glacial periods.

It was shown that all the hypotheses thus far advanced to explain the glacial periods fail of correspondence with the known facts of geology, particularly the discovery of glacial periods earlier than that of the Quaternary. The kinetic theory of gases applied to the atmospheres of the planets was found to invalidate the La Placian theory of the universe as well as the later theory of meteoric swarms. The atmosphere being the blanket of the earth the explanation of the earth's warm and cold periods was sought in the variation of the amount of carbon dioxide contained, which would materially affect screening or blanketing capacity. Alternate depletion and repletion of the carbon dioxide of the atmosphere might be caused by the variation in the size of land areas and the consequent variation in the carbonation of rocks, and by the separation of carbonates from the littoral portions of the seas. Mr. Chamberlin discussed a theory of the origin of the earth by the slow accretion of solid matter from without, a theory in contrast with that of La Place, in that instead of beginning with an enormously heavy atmosphere, on his theory an atmosphere could not exist until the earth was about one-tenth grown, and would then be extremely attenuated to increase in density with the enlargement of the planet. The exceeding difficulty of securing data for calculation in this field was emphasized, as was the necessity of submitting not one but a number of working hypotheses to the most searching of tests.

Wm. H. Hobbs.

ZOOLOGICAL CLUB, UNIVERSITY OF CHICAGO, MEETINGS OF DECEMBER, 1899.

The session of December 15th was devoted to a paper by Mr. M. F. Guyer on 'Spermatogenesis of Hybrid Pigeons.' "In the spermatogenesis of hybrid pigeons several abnormalities are manifested. These may be classified conveniently under three heads: (1) abnormalities in the structure of the spermatozoa; (2) abnormalities in mitoses; (3) degeneration of the germinal cells. Abnormalities in the spermatozoan structure were present in sterile hybrids, the most noticeable feature being a varicosity or swelling about the middle of the head. In tracing the development of the spermatozoa, this curious modification was found to be due apparently to a lack of development of the head; the nucleus did not elongate completely as in normal spermatogenesis. Abnormalities in mitosis were marked in both fertile and sterile hybrids. Large numbers of multipolar spindles were present. These were usually of the tripolar type. Occasionally two distinct and separate spindles occurred in one cell. The spermatocytes of the first order were the the cells that showed this phenomenon to the greatest extent. In the normal pigeon the chromosomes in the spermatogonia are sixteen in number and in the primary spermatocyte eight. The latter are laid down in rings and each is evidently double. On the spermatogonia of the hybrid there were sixteen chromosomes and in the primary spermatocytes often more than eight. In the latter there may be several of the large double type and a number of smaller rings, or sixteen small ring chromosomes may occur. If sixteen rings were present they were usually located on two separate spindles, eight to each spindle. Another peculiarity in the mitosis was the frequent inequality in the division of the chromosomes, in some instances only about a fourth of a chromosome going to one pole. It is a well known fact that the descendants of hybrids are remarkably variable, hence the possibility that this irregularity in chromatin distribution of the parent germ cell and the variability of the offspring may be connected in some way immediately suggests itself. As for the degeneration of the germ cells, this phenomenon was observed in sterile birds only."

At the meeting of December 20th, Dr. E. S. Riggs, of the Field Columbian Museum, contributed an illustrated lecture, 'The Fossil Mammals of North America.' A large number of lantern slides were exhibited; among them many reproductions in color of the restorations of fossil mammals executed by Mr. Knight.

C. M. Child.

BIOLOGICAL SOCIETY OF WASHINGTON.

THE 317th meeting was held on Saturday, January 27th. William Palmer exhibited a series of specimens of the Christmas Fern, *Polystichum acrostichoides*, showing variations in growth due to varying conditions of environment. H. J. Webber showed a portion of one of the negatively geotropic roots of the mangrove, *Rhizophora mangle*, stating that their function, like that of the 'knees' of the cypress was that of aëration, and illustrating his remarks with photographs.

Thomas A. Williams presented some 'Notes on a New Species of *Lecidea* from Mexico,' stating that it was related to *L. speirea* Nyl. It is peculiar on account of its *Lecanora*-like fruit, the apothecia presenting a white border when young and being borne on slight elevations of the thallus, after the manner of the species of Bæomyces with sub-sessile fruits. In some instances gonidia were found in the apothecia, but their occurrence seemed to be accidental rather than normal. The bearing of these structural peculiarities on the systematic relationship of this and other *Lecideas* was discussed.

Barton W. Evermann made 'Some Observations concerning Species and Subspecies', basing his remarks upon the species of darters found in the waters of Lake Maxinkuckee and a new species found in Aubeenaubee Creek, the principal tributary of the lake, but not in the lake itself. This fish is evidently derived from *Etheostoma iowæ*, which is found in the lake, but differs from it clearly and constantly, and no intergrading forms are known. Should, for some reason the lake species and that from the creek invade each other's habitat and interbreed, the result would be the production of individuals possessing characters common to the two species and apparently placing them in the relation of species and subspecies, although if this supposed case were definitely *known* to have occurred, we should regard the individuals as hybrids and the other forms as still distinct species. The speaker reviewed the chief categories of subspecies, concluding that many trinomials were in use where a careful examination of the facts would show that the supposed subspecies are really species. The paper will appear in SCIENCE.

T. W. STANTON,
Secretary.

NEW YORK ACADEMY OF SCIENCE.
SECTION OF ANTHROPOLOGY AND PSYCHOLOGY.

THE regular meeting of the Section was held on Monday, January 22d. The first paper of the evening, entitled 'Some Phenomena of Indirect Vision,' was presented by Mr. Clark Wissler. Experiments were made by exposing in the indirect field of vision, while the attention of the observer was fully occupied with objects in the direct field, small letters or numerals. The subjects were not conscious of seeing the characters presented in the indirect field, indeed, one of the subjects whose results were reported did not know until several tests had been made that the characters were there at all. In spite of this failure to receive conscious impressions from the letters and figures, two subjects were found who could by association, and afterwards by memory, give in a large number of cases the correct numeral or letter. These subjects made their associations in the form of visualized images. Many of the errors made were similar to those made in normal vision. Thus, *c* was mistaken for *o*, *35* for *55*, etc. It was held that the experiments suggest a relation between normal phenomena and the abnormal as seen in the hysterical eye, and that they point out a way to more complicated experiments in induced automatic movements.

Professor J. McK. Cattell presented a paper on 'The Relations of Time and Space in Vision.' He described experiments on the perception of moving surfaces, which show that a time series may be perceived as a spacial continuum, and explained that the same phenomena held in the ordinary vision of daily life. Although the eyes, head and body are in continual movement, and the images on the retina are constantly shifting, the field of vision appears to be distinct and stationary. Thus if one glances along a row of books, images follow one another on each retinal element in rapid succession, but these successive and rapid changes result in the perception of a space continuum, all the objects being distinct and arranged side by side.

Professor Buchner read a paper on 'Number Forms.' The paper described with the aid of sketches the fixed visualizations experienced since childhood by a woman 35 years of age.

There are three distinct, uncolored tridimensional forms. The first is half fan-like in shape, lying almost entirely to the left of the mental point of regard, and includes the numbers from 1 to 100. The second includes the names of eight days, from Sunday to Sunday. The third has the names of the twelve months from January to December. The paper pointed out the elements which must appear in any theory of the genesis of the phenomena to which this group belongs.

Charles H. Judd,
Secretary.

CURRENT NOTES ON PHYSIOGRAPHY.

WESTERN NEBRASKA.

A report on the geology and water resources of the westernmost twelfth of Nebraska, by N. H. Darton (19th Ann. Rept. U. S. Geol. Survey, pt. IV., 1899, 721–785, numerous maps and illustrations), presents a very clear picture of an interesting region. The inter-stream areas are generally plateau-like uplands of Tertiary strata, retaining something of their initial smoothness of surface over considerable distances. The sand-hill area of mid-western Nebraska extends west into the broad upland between the North Platte and the Niobrara, where some east-flowing streams are lost. The chief valleys, that of the North Platte being the largest, are cut by streams whose courses seem to be consequent on the general easterly slope given to the region when it was uplifted. Numerous branch streams of unsystematic (insequent) arrangement dissect the valley sides, often producing characteristic bad-lands. The insequent dissection has gone so far between North Platte river and Pumpkinseed creek as to reduce an upland to a narrow ridge with numerous lateral spurs. Pine ridge, trending east and west near the northern border, is the strongest relief in the State; it is a cuesta-like upland whose escarpment is carved into bad-lands by its obsequent streams which descend northward to a denuded area of Cretaceous strata that border the southern flank of the Black hills. The present relation of ground water, springs and streams to a structure and form are well set forth in the later pages of the report. The same author contributes a brief account of the Bad Lands of South Dakota and Nebraska to the September number of the *National Geographic Magazine.*

Mention may be made in this connection of an article by W. D. Matthew on the interpretation of the White river Tertiary strata of Nebraska and South Dakota as an æolian instead of as a lacustrine formation (*Amer. Nat.*, xxxiii., 1899, 403–408).

THE MISSISSIPPI AND MISSOURI RIVERS.

The annual reports of the commissions on our two greatest rivers (Apps. WW and XX., chief of engineers, United States Army, Washington, 1899) contain a large amount of interesting matter, whose discovery would be much facilitated if the reports were edited with more consideration for their readers. Hundreds of pages without adequate tables of contents and with unchanged page headings make the use of the reports difficult. Numerous measured sections of the Mississippi lead to the conclusion that if the banks are properly revetted to prevent erosion, while levees on the adjacent floodplain restrain the spread of high waters, the channel will be deepened and its capacity to discharge floods increased. The Yazoo basin has 310 miles of levees to protect 7100 square miles of surface. Much money has been spent on the levees by local authorities, and yet it is estimated that the volume of the levees must be increased by more than half in order to bring them up to the proper size. $20,000,000 will be needed to complete the entire levee system; over $2,000,000 having been spent in 1899. The heights of floods, their progress down the river, the locations of levees and areas of overflow are shown on various plates and maps. Besides a new edition of the famous eight-sheet map of the lower Mississippi, a four-sheet map of the upper part of the river was issued during the past year on a scale of 1 : 316,800. No relief is indicated except along the borders of the floodplain, but this suffices to suggest that the valley is the work of a larger river than that now flowing in it; not merely because the valley is wider than the river, but because the curvature of the valley is of a larger pattern than the present river seems capable of producing. The narrow postglacial rock-walled channel just above Keokuk

is well shown in relation to the broader valley up and down stream. Five new sheets of the detailed charts, 1 : 20,000, were issued during the year.

The report on the Missouri announces that it is impracticable to attain permanently useful results in controlling the river at the present rate of expenditure; $317,000 having been spent on river corrections during the year. Along with numerous maps representing various engineering works, the report contains a large number of excellent photographs illustrative of different methods of protecting the river banks, from which an excellent idea of the appearance of the river and of the works undertaken upon it can be gained. A series of detailed charts covering the river from its mouth to Kansas City (400 miles) on a scale of five inches to a mile with five-foot contours, have been drawn but not yet published.

GLACIAL LAKE OUTLETS IN MICHIGAN.

THE 'thumb' of Michigan, enclosing the Saginaw bay branch of Lake Huron on the southeast, is of moderate altitude, yet sufficient to have divided two lobes of the ice sheet of the last glacial epoch, which deposited their interlobate moraines along the axis of the thumb. During the retreat of the ice, the depressions evacuated by these lobes were occupied by lakes; the southeastern by Lake Maumee, overflowing through the outlet past Fort Wayne, long ago described by Gilbert; the northwestern by Lake Saginaw, whose outlet was through the Grand river channel, a magnificent ancient river bed, a mile wide, fifty miles long and sometimes cut over 200 feet deep in the drift. Further retreat of the ice uncovered a point on the crest of the thumb of less altitude than the Fort Wayne outlet; then the southeastern lake drained across the thumb to the northwestern lake, the connecting river carving the Ubly channel, which follows the outer base of an interlobate moraine. The channel is twenty miles long, a mile wide, and from 20 to 100 feet deep. At its southeastern end, its level agrees with that of the shore lines of the lake that it drained; its bed is strewn with bars of gravel and sand, indicating a flow from southeast to northwest; its further end opens upon a delta-like body of gravel at the level of Lake Saginaw. Like the ice-border channels near Syracuse, N. Y., discovered by Gilbert, or those of north Germany recently summarized by Keilhack, the Ubly channel is a geographical feature of marvellous significance in connection with the glacial theory; the interpretation of this excellent example being due to Taylor, in whose admirable series of independent studies it constitutes but one of many items (Ice dams of Lakes Maumee, Whittlesey and Warren, *Amer. Geol.*, xxiv., 1899, 6–38, maps).

CHICAGO AND ITS ENVIRONS.

THE first Bulletin of the Geographic Society of Chicago contains an essay on the 'Geography of Chicago and its Environs' by Salisbury and Alden (pp. 64, 30 figs.). A relief plate as frontispiece shows very clearly the smooth floor of the ancient expanded lake rising towards the rolling uplands through which the lake outlet cut its broad and well-defined channel. The text describes the several physiographic areas, with special reference to the successive stages of the falling lake, of which three are recognized (Glenwood, Calumet, Tolleston). The dunes of the ancient beach ridges that curve around the southern end of Lake Michigan, familiar objects to travelers by rail from the east, are mapped and described.

W. M. DAVIS.

CURRENT NOTES ON METEOROLOGY.

COMPENSATION IN WEATHER.

THE question of seasonal forecasts is considered in the Annual Summary for 1899 of *Climate and Crops: Colorado Section.* The temperature and precipitation data for Denver during the past 28 years have been compiled in order to bring out whatever relation successive seasons bear to one another, in the hope of throwing some light upon the so-called theory of compensation in weather. This theory, stated in a few words, is that a season with an excess or defect of temperature or precipitation is followed by compensating conditions in the succeeding season. The records show that the temperature for a season, or a longer period, furnishes no certain index of the conditions to be expected during the coming season. An ex-

ceptionally warm spring or summer following an abnormally cold winter is found to be the exception rather than, as is generally believed, the rule. The conditions with respect to precipitation are much more variable than those connected with the temperature. Notably dry or wet seasons are more likely to be followed by nearly normal ones, than by seasons having compensating, or opposite, characteristics.

In *Nature* for January 25th, MacDowall contributes a further note to this discussion. The subject of this inquiry is the sort of relation subsisting between the cold of a given winter and that of the 30 winters preceding. The cold of the winter seasons is measured by the number of frost days from September to May. The results of the study are as follows: (1) The six mildest winters (since 1871) were each preceded by a 30-year group having more than the average of frost days. (2) The six coldest winters were each preceded by a 30-year group having less than the average of frost days. (3) Of fifteen 30-year groups with excessive cold (*i. e.*, over the average), as many as 12 were followed by mild winters, and only 3 by severe winters.

Studies of the sort here referred to are always interesting, but it must be remembered that the results, so far as they go, relate only to a limited area in each case, and that no definite general conclusions can be reached in this matter without much longer and much more accurate series of observations than we now have.

MONTHLY CLIMATE AND CROP BULLETIN.

The *Climate and Crop Bulletin* of the Weather Bureau for January contains a new feature. This is the addition of a diagram indicating the average daily departure from normal temperature for each day during the month at certain selected Weather Bureau stations east of the Rocky Mountains. These stations are St. Paul, Galveston, Boston, Jacksonville, and Cincinnati. These five cities are believed to represent the general temperature conditions prevailing east of the Rocky Mountains as well as any other like number of stations. Simple graphic representations are always welcome additions in discussions of meteorological phenomena, and this new diagram is certain to meet with approval on the part of all who make use of the *Climate and Crop Bulletin*.

R. DeC. Ward.

Harvard University.

THEODORE POESCHE.

On December 27, 1899, died in Washington, D. C., Theodore Poesche, one of that coterie of scholars of whom Professor Henry said, no one has ever asked me a question that some of them could not answer correctly. Poesche was born at Zoeschen, near Merseburg, graduated at the University of Halle, and was driven to England for participating in the revolution of 1848.

Coming shortly after to America, he published with the coöperation of Carl Copp, a little book entitled 'The New Rome,' in which a comparison is drawn between the hereditary enmity between Rome and Carthage on the one hand and between England and America. In 1857 Poesche came to Washington, where during forty years he served as statistician in the Treasury. In this capacity he was sent in 1872 to advise Bismarck about the working of our internal revenue system. In 1878 appeared his masterpiece, 'Die Arier,' in which the origin of the blonde Aryans, of whom Poesche was a splendid example, is found in the Rökitno marshes of White Russia. The book is a protest against the Asiatic origin of the blondes and contributed no little at the time to change the prevailing opinion. In all his work Mrs. Poesche was the amanuensis of her husband and occupied a prominent place in the Washington literary circle.

O. T. M.

SCIENTIFIC NOTES AND NEWS.

At the meeting of the Royal Geological Society on February 16th, Mr. Henry White, Secretary of the United States Embassy, received on behalf of Mr. G. K. Gilbert, the Wollaston Medal.

Lord Rayleigh, Professor Ramsay, Dr. W. Hittorf and M. Moissan have been elected honorary members of the German Chemical Society.

The polling for the election of a member to represent the University of London in Parlia-

ment at last accounts had reached: Sir Michael Foster, 903; Dr. Collins, 662; Mr. Busk, 439.

PROFESSOR MAX VON PETTENKOFER, of Munich, eminent for his contributions to hygiene and sanitation, has been elected a Knight of the Prussian Order Pour le Mérite in the Section of Sciences and Arts.

THE following named medical gentlemen have accepted non-resident membership in the Washington Academy of Sciences: H. P. Bowditch, R. H. Fitz, Boston; A. Jacobi, E. G. Janeway, T. Mitchell Prudden, G. S. Huntington, New York; W. W. Keen, Philadelphia; H. A. Kelley, Wm. Osler, Wm. H. Welch, Wm. S. Halsted, Baltimore; Nicholas Senn, Chicago; P. S. Conner, Cincinnati.

DR. J. B. HATCHER, of Princeton University, whose appointment to the curatorship of paleontology we announced last week, will begin his work at Pittsburg on March 1st.

THE Turin Academy of Sciences has awarded the Bressa Prize of 10,000 lire for the best scientific work published during the past four years to Professor Ernst Haeckel, of Jena.

THE Berlin Academy of Sciences has elected to membership Dr. Wilhelm v. Branco, professor of geology and paleontology in the University.

THE Munich Academy has awarded its great gold medal to the explorer, Eugen Wolf.

THE Academy of Sciences of St. Petersberg has elected Professor Fischer of Berlin, and professor Boltzmann of Vienna, as corresponding members.

THE University of Berlin has conferred an honorary degree of doctor of philosophy on Ignaz Stroof, the chemist at Griesheim.

A BUST of Bernhard von Langenbeck, the eminent surgeon, is to be placed in the great hall of the University of Berlin.

DR. HANS BRUNO GEINITZ, a geologist and paleontologist of distinction, died at Dresden on January 28th in his 86th year.

WE regret also to record, on December 9th, the death of Walter Götze, the botanist, while on an expedition to German East Africa.

THE Highland Agricultural Society has contributed £200 in aid of Professor Ewart's experiments on telegony. A gift of £50 has also been promised by Sir John Gilmour.

A COLLECTION of invertebrate fossils from Tennessee has recently been purchased for the Peabody Museum, Yale University. A rearrangement of many of the specimens in the museum is in progress, and some heretofore inaccessible have been placed on exhibition. The exhibits are to be photographed systematically for an illustrated catalogue.

THE collection of Indian relics which was on exhibition at the Boston Museum for many years, has been presented to the Peabody Museum of Harvard by the heirs of David Kimball. The collection comes from the Algonquins, the Sioux, the Seminoles and the Choctaws, and was made by the famous explorers, Lewis and Clark, about the year 1840.

THE trustees of the Western University of Pennsylvania have decided to begin the erection of the new building of the Allegheny Observatory. It will be erected in River View Park and cost $250,000.

THE St. Petersberg Institute of Experimental Medicine has established a laboratory for the study of plague and for the manufacture of plague serum at Cronstadt. The laboratory is surrounded on all sides by water, so that it can be completely isolated.

THE Brooklyn Institute of Arts and Sciences receives $15,000 by the will of Joseph C. Hoagland.

THE President of the Royal College of Physicians has announced that the annual Harveian Oration would be delivered by Dr. T. Clifford Allbutt, regius professor of physics in the University of Cambridge, that Dr. Archibald Garrod has been appointed Bradshaw Lecturer, and Dr. John Sykes, M.O.H. St. Pancras, Milroy Lecturer for the ensuing year (1901).

THE Washington Academy of Sciences announces that the second of the series of meetings for the exposition of 'Photography as an Aid to Research' will be held at Columbian University at 8 p. m., Thursday, February 15th, when the following topics will be presented: 'Photography as an Aid to the Study of Plants,' by F. V. Coville; 'Photography in

the Study of Animal Physiology and Pathology,' by Dr. D. S. Lamb, and 'Photography as an Aid in Medicine and Surgery,' by Dr. W. C. Borden, U. S. A. These will be illustrated by lantern slides. At the succeeding meetings, to be announced later, the following topics will be presented: 'Photography in the Study of Animals; in Geographic Research and Survey; Geology, Paleontology, Astronomy, Physics, Physical and Criminal Anthropology, Ethnology, Archæology, Literature and History.'

The School of Pedagogy, New York University, has announced a special course of lectures on Education, to be given on Monday evenings in March, as follows: March 5th, 'Physical and Mental Growth of Children between the Ages of Six and Twelve Years,' Professor Edward R. Shaw; March 12th, 'Education as a Scientific Pursuit,' Professor Edward Franklin Buchner; March 19th, 'A Fundamental Principle of Mental Development,' Professor Charles H. Judd; March 26th, 'Ethics as determining the End of Education,' Professor Samuel Weir.

The Society of German Men of Science and Physicians will meet at Aachen from the 17th to 21st of September of the present year.

The New York Academy of Sciences will hold its annual meeting on Monday evening, February 26th. The program includes reports of officers for past year: Election of officers, honorary members, corresponding members and fellows; followed by the Presidential Address, by President Henry F. Osborn, entitled 'The Geographical and Faunal Relations of North America, Asia, and Europe during the Tertiary Period.'

The third annual meeting of the National Pure Food and Drug Congress will be held in Washington on March 7th and following days. The meeting is regarded as of special importance as it may increase interest in the National Pure Food and Drug Bill now before Congress.

An International Congress of Ethnographical Science will be held at Paris from the 26th of August to the 1st of September of the present year. There will be seven sections as follows: general ethnology, sociology, psychology religions, linguistics, sciences and arts and descriptive ethnology. The president is M. Maurice Block and the general secretary is M. Georges Raynaud, rue Mouffetard 82, Paris.

The fifth annual mid-winter meeting of the Vermont Botanical Club was held at the University of Vermont, January 26th and 27th. The officers of the club were reëlected: President, Ezra Brainerd; vice-president, C. G. Pringle; secretary and treasurer, L. R. Jones.

A bill has been introduced into the Assembly of the State of New York, providing for a biological station with the objects noted in the last issue of this Journal. The management of the institution is under a board of control, consisting of the New York state fish culturist; the president of the board of the commission of fisheries, game and forest of the state of New York; the president of the New York state fish, game and forest league; the chief of the bureau of nature study of the college of agriculture of Cornell University; and the director of the New York state college of forestry, thus making the board of control to consist of five members. The sum of $10,000 annually is appropriated for the expenses of the station.

An account was quoted from the London *Times* in last week's issue of Science of the work done by Italian students in investigating malaria. Dr. Ronald Ross claims that he was done an injustice, as he had anticipated the Italian investigators in following out the life history of the Hæmamœbidæ (the group of parasites of which the human varieties cause malarial fever) in mosquitoes.

A very valuable machine for cutting minerals, called a *petrotome*, invented by Professor William B. Dwight, of Vassar College, has recently been made accessible for general scientific and commercial uses. Dr. A. E. Foot, of Philadelphia, is to have a large collection of minerals at the Paris Exposition and will exhibit a petrotome in action, to illustrate the best scientific method of cutting rocks and precious stones. Half a dozen large transparent sections have been made by Professor Dwight to be sent to Paris with the machine. One of these is a fossil solidified trunk of a tree. It is seven and a-half inches in diameter and is cut so thin

throughout as to show perfectly the microscopic structure of the wood. Another specimen is of a group of Rubelite crystals embedded in Lepidolite, and a third is a section of transparent green serpentine five by two and a-half inches in size. A petrotome has lately been secured by the Geological Commission of Brazil and one by Yale University for their scientific work.

IN spite of the prevalence of typhoid fever in Philadelphia the death rate is lower than usual. The Bureau of Health has given out the following figures for the past twelve years:

	Deaths.	Rate per 1000.
1888	20,372	20.04
1889	20,536	19.74
1890	21,730	20.76
1891	23,367	21.85
1892	24,305	22.25
1893	23,655	21.20
1894	22,680	19.90
1895	23,796	20.44
1896	23,892	20.17
1897	22,735	18.72
1898	23,790	19.18
1899	23,796	18.78

BY a recent decree of the Russian Minister of Education the admission of first-year students by the several medical faculties throughout the empire is restricted to a fixed number. The University of Moscow is limited to 250, Kieff to 200, Charkow to 175, Dorpat to 150, Warsaw to 100, Tomsk to 120, and Kasan to 100. The total number of first-year medical students in Russia must therefore not exceed 1095. This number does not include the students of the St. Petersberg Medico-Military Academy, which may admit 250 first-year students.

ACCORDING to the *Publishers' Weekly*, there were last year published in the United States 176 books in the sciences as compared with 143 in 1898. These numbers do not, however, include educational or medical books, or books in the 'useful arts.'

AT a special meeting of the London Chemical Society of London, Professor T. E. Thorp, F.R.S., gave a memorial lecture in honor of Victor Meyer. He said, according to the report in the London *Times*, that as a friend of nearly 30 years' standing, and as one who studied with him under Bunsen, he had acceded to the request of the council to record its appreciation of the remarkable services rendered by Meyer to the science he cultivated with such assiduity and success. After an account of Meyer in his student days at Heidelberg and of his work as one of Bunsen's assistants, the lecturer told how in 1868 he entered Baeyer's laboratory in Berlin, where his success as a private teacher procured him the position of assistant to Fehling at the Stuttgart Polytechnic. There he made one or two important discoveries, but in less than a year he was called, when barely 24 years of age, to succeed Wislicenus at Zürich. His 13 years' stay there constituted the most fruitful and brilliant period of his career, and before its close he had brought himself within the foremost rank of contemporary investigators. Some idea of his power of work and of the stimulus he gave to others might be gleaned from the fact that during that period the Zürich Laboratory gave close on 130 papers and memoirs to chemical literature. It was at Zürich, too, that he devised his various methods of determining vapor densities and carried out some of his work on the dissociation of the halogens. Pyrochemical problems always interested him, and he expressed the belief that a new chemistry with new and undreamt-of discoveries would disclose itself when vessels were obtained capable of bearing temperatures at which meter could no longer exist and oxyhydrogen gas became an uninflammable mixture. In 1882, when continuing a series of University lectures on benzene derivations which had been interrupted by his friend Weith's death, he made what was, perhaps, his most brilliant discovery, that of thiophen, and within about six months of his first observation he was in a position to show by actual preparations that its chemistry was hardly less extensive than that of benzene itself. On the death of Hübner he was called to Göttingen, and in 1888 to Heidelberg, as successor to Bunsen, with the coveted title of Geheimrath and the promise of a new and enlarged laboratory. In conjunction with a number of his pupils he there began the investigation of the conditions determining both the gradual and explosive combustion of gaseous

mixtures, and in 1892 with Wachter he announced the existence of the iodoso-compounds, the study of which led to the discovery of the remarkable substances known as the iodonium bases. The formation and electrolysis of ethereal salts of aromatic acids occupied him from 1894 up to the year of his death, which took place at the age of 49 on August 8, 1897. To the literature of chemistry, either alone or in conjunction with his pupils, he contributed over 300 memoirs and papers. As the director of a large chemical laboratory and as a laboratory teacher he worthily followed in the footsteps of Bunsen. He was an admirable lecturer, clear and vigorous, while as a teacher he had a wonderful power of infusing enthusiasm into his students. His literary ability, combined with his faculty of exposition, made him an admirable writer of popular science articles.

UNIVERSITY AND EDUCATIONAL NEWS.

Mrs. Thomas McKean has given $250,000 to the University of Pennsylvania towards the cost of the new Law School building.

Boston University receives $50,000 by the will of O. H. Durrell, of East Cambridge, Mass.

A half million dollars will be distributed by Dr. D. K. Pearsons of Chicago, beginning March 1st, among fourteen colleges throughout the United States. Most of the donations are made on condition that the colleges raise a certain amount, generally $50,000, or an amount equal to the gift, within a given time. The first college to claim its proportion of the $500-000 is Mount Holyoke College, South Hadley. This college receives $50,000, and the gift will be made March 1st. Some of the other colleges to become beneficiaries of Dr. Pearson's philanthropy are Yankton College, South Dakota; Berea College, Berea, Ky.; Colorado College, Colorado Springs, Col., and McKenzie College Lebanon, Ill., which will receive $50,000. Each has received a former gift from Dr. Pearsons. Dr. Pearsons has already given $2,500,-000 to the cause of education.

The Baltimore Association for the Promotion of the University Education of Women is prepared to offer a foreign fellowship of the value of $500 for the year 1900–1901. Preference will be given in the award of this fellowship to women from Maryland, or women who have identified themselves with educational interests in Maryland.

A school of forestry will be established at Yale University under the Sheffield Scientific School. It will occupy the house left to the University by the late Professor Marsh.

The proposed changes in the examinations for the Mathematical Tripos at Cambridge, abolishing the order of Merit and the Senior Wrangler, have been defeated in the Senate by a vote of 152–139.

Plans are being made for the establishment of a school for scientific instruction and practical training in agriculture and horticulture, to be situated at Chappaqua, 33 miles from New York city. The students would attend lectures and do work in the New York Botanical Garden, which is easily accessible.

It is reported in the daily papers that Mr. Alexis E. Frye, superintendent of schools in Cuba, is arranging for a number of Cuban teachers to attend the Harvard summer school.

It is announced that Rear Admiral William Sampson has been offered and has declined the presidency of the Massachusetts Institute of Technology.

Dr. Samuel J. Barnett, professor of physics in Colorado College, has been appointed assistant professor of physics in the Leland Stanford, Jr. University.

Dr. Edward Lewis Stevens, B.A. (La. State University), Ph.D. (New York University), has been appointed president of the newly established Louisiana State Industrial School.

Dr. W. Wien, professor of physics at the University of Giesen, has been called to Würzberg, and Dr. Ludwig Knor of Jena, has been called to the professorship in chemistry in the University of Freiburg, i. B. Dr. Winkler has been made assistant professor of agriculture in the Agricultural Station at Vienna, and Dr. F. R. Kjellmann, professor of botany in the University of Upsala. Dr. Ebermeyer, professor of agriculture and meteorology at Munich, has retired.